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*SOME ADVANCES MADE IN ASTRONOMICAL SCIENCE DURING THE NINETEENTH CENTURY.**

IN glancing over the field of astronomical achievement during the century recently closed, the most striking feature is undoubtedly the rise and growth during the past fifty years of an entirely new department, one totally unknown and almost undreamed of before the first half of the century had passed, that called by Professor Langley the new astronomy, more technically known as astrophysics. In considering the restrictions which must be regarded in case this address is to be kept within manageable limit, perhaps it would be well to confine my remarks to this new branch of the science. I shall, however, give the chief place to the older astronomy, touching briefly upon the newer phase.

Near the close of the seventeenth century appeared Newton's immortal work called by him 'The Mathematical Principles of Natural Philosophy.' In this treatise the law of universal gravitation as the controlling and governing principle of the planetary system was established by a rigorous course of mathematical reasoning. It was many years, however, before these conclusions were universally accepted. On the continent of Europe particularly, the

* Annual address delivered before the University of Pennsylvania Chapters of the Society of Sigma Xi, June 13, 1901, by C. L. Doolittle.

field was already occupied by the philosophy of Descartes. The glittering generalities found here in place of the severe mathematical reasoning of Newton proved too attractive to be at once overthrown.

Fifty years elapsed before anything material was added to the science of Newton, then came a galaxy of distinguished men, including Euler, Clairaut, D'Alembert, Lagrange and Laplace, who, by a series of most brilliant and exhaustive researches, made possible by the development of much more powerful mathematical instruments than those possessed by Newton, practically disposed of every objection which the opponents of Newton's theory could discover.

Their last stronghold and one which proved extremely difficult to carry was the explanation of the secular acceleration of the moon's motion. Halley, one of Newton's disciples, had found by a comparison of ancient and medieval eclipses with those of modern times, that the lunar month is now shorter than was the case 2,000 years ago. How this could be if the motion of this body were governed by the law of gravity only proved an extremely difficult question. It was finally shown by Laplace to be one of the consequences of this law, that what is now an acceleration will in the future become a retardation, thus preserving the system essentially as we see it to-day. This took place near the beginning of the nineteenth century. We may, therefore, say that this century began its course with the law of universal gravitation firmly established. In fact no one was hereafter found to seriously call it in question whose opinion was worthy of notice.

It is not to be supposed, however, that the science of celestial mechanics was now finished. In fact we can hardly say that any branch of physical science ever has been or ever will be finished. Results depending for their value upon observations of any kind must share the inevitable imperfections of the observer and of his instrument.

The problems of celestial mechanics have accordingly occupied the attention of a large number of distinguished men during the century past. With the discovery of new planets and comets and stellar systems, new applications are constantly appearing calling for the most refined skill and active perseverance of its votaries. Refinements in observation and improvements in method also call for frequent revision of the old investigations. This is particularly true of the lunar theory.

The study of the moon's motions was one of the first problems to attract the attention of the ancient astronomers. Since their day we are probably safe in saying that no problem in the entire range of science has called out anything approaching the labor and ingenuity bestowed upon this one. Yet the theory is hardly in a satisfactory condition to-day. Such a statement may seem to those who have never looked into the intricacies of the problem like a confession of failure.

So far as it concerns a purely mathematical statement of the conditions the problem offers no difficulties. The relations between the coordinates of a body like the moon, acted on by any system of forces, are very readily expressed by a series of three differential equations of the second order. The forces here are the mutual attractions exerted by the sun, moon and planets. If only two bodies are present the solution offers no difficulty. If there is a third, we have the famous problem of the three bodies. In spite of all that has been done during the century in the way of mathematical advancement this problem still defies its most powerful resources. Recourse must be had to methods of approximation, expansion in the form of series being one of the most obvious. No one can form an idea of the intricacy and

complexity of the resulting expressions who has not himself looked into the problem. Delaunay, at one time director of the Paris Observatory, carried this investigation farther than any of his predecessors had done, but though it had occupied his almost undivided attention for twenty years, it was still unfinished at the time of his death. (He was unfortunately drowned at Cherbourg in 1872 by the capsizing of a pleasure boat.) We have in this country to-day three investigators who are perhaps the greatest living authorities on this subject—Professors Newcomb, Hill of New York, and Brown of Haverford.

The general problem of three bodies has proved a very fascinating one. If, for instance, Mars and Jupiter were of approximately the same dimensions as the sun, the determination of their respective motions would be vastly more complex than it now is. Mr. G. H. Darwin has been giving much attention to this class of problems bringing out some interesting results. Although the problem does not confront us in this unmanageable form in our own system it will eventually find practical application in unraveling the intricacies of the stellar motions.

Probably no achievement in the domain of science ever produced so profound an impression upon the unprofessional public as the discovery of Neptune by a purely theoretical investigation undertaken independently by Leverier and Adams, predicting its place in the heavens before it had ever been seen, or at least recognized as a planet. It is unnecessary now to rehearse the familiar story of that brilliant discovery. It was somewhat like the egg of Columbus, but up to the present time the attempts to apply the process in other regions of the planetary system have not been attended with success. The first such attempt was by Leverier himself, followed in 1859 by his confident announcement of a planet between Mercury and the Sun. The prestige of Leverier's name, accompanied by a supposed view of the planet by Dr. Lescarbault, an amateur astronomer, proved sufficient to carry conviction generally; and Vulcan found a place with the other planets in many books written thirty or forty years ago. No one else, however, whose authority was worth much claimed to have seen the planet until the occurrence of the total eclipse of the sun July 29, 1878. At this time Professor Watson, of Ann Arbor, and Lewis Swift, of Rochester, believed they had each seen one and perhaps two planets near the sun. The reputation of both as skilled observers naturally gave great weight to their authority, but the planets were never seen again and a critical examination of the places assigned renders it practically certain that fixed stars were mistaken for planets. Vulcan as a single large planet within the orbit of Mercury is now by the unanimous verdict of the astronomical profession relegated to the realm of myths. It was very natural to attempt to apply the process to the discovery of planets beyond Neptune, but although some indications of one and possibly two such have been suspected, nothing of the kind has yet been seen.

The eighteenth century ended with a list of known planets numbering 7 not including satellites. The nineteenth began with eight, the first day of January, 1801, being distinguished by the addition to the list of Ceres, the first of the long line of asteroids to be detected. The fortunate discoverer was Gieuseppe Piazzi. The scene of Piazzi's activities was the University of Palermo, where he had been diligently engaged for nine years with the most perfect instrument which the skill of that day could produce in accumulating materials for a great stellar catalogue. At the time of which we are speaking his attention was directed to a place in the constellation Taurus on account

of an error of Wollaston, one of his contemporaries, in assigning a place to a star where none existed. Piazzi found, however, an eighth magnitude star not before noted, and upon repeating the observation the following evening, it was found to be in motion. Piazzi carefully followed the planet until February 11, when he fell ill, and his activities were for some time suspended. Meanwhile letters had been sent to Oriani, at Milan, and Bode, at Berlin. About this time, however, it may be remembered that another individual, not wholly unknown to fame, was actively employed in this part of the world in the practical applications of military science. We refer to one Napoleon Bonaparte. This may explain the fact that Oriani's letter arrived at Milan two and one-half months after it was written. Olbers, however, received that directed to him at the end of two months. The planet was now lost in the sun's rays and it was greatly feared that it would not be recovered, for. with a body so minute, unless its position could be given with some approach to accuracy the attempt to find it was an almost hopeless task. This emergency brought to the front the great mathematician Gauss. At that time a young man unknown to fame, he attacked the problem, and as a result produced a method for determining an orbit from three observed positions which completely overcame the difficulty, at the same time showing its author's title to a place in the front rank of mathematicians.

The discovery of three more planets belonging to the asteroid group soon followed, Juno, Vesta and Pallas, then, after a long interval came Astræa in 1845. The discoverer was Hencke, an amateur astronomer, who had been watching the heavens during fifteen years in the hope of this reward. The number now known to exist is near five hundred, with no indication that the supply approaches exhaustion. About ten years ago the application of photography to this purpose by Wolf, of Heidelbegr, made possible what may be called a wholesale process. One plate, taken October 22, 1900, showed no less than five of these bodies. This is at present believed to be the maximum record. What is to become of this numerous family is one of the serious questions of this day. The complete investigation of the theory of one is almost the work of a lifetime.

Of satellites, or secondary planets, seven have been added to the list during the century, one of Saturn, by Bond of Cambridge, September, 1848; one of Neptune, by Lassell, soon after the discovery of the planet itself; two of Uranus, also by Lassell, 1851; two of Mars, by Hall, of Washington, August, 1877; one of Jupiter, by Barnard, of the Lick Observatory, September, 1892. We should perhaps include an eighth in this category, an additional satellite of Saturn having been announced by W. H. Pickering two years ago, but as it has not yet been confirmed, judgment must be suspended for the present.

The periods of rotation of Mercury and Venus were investigated by the German astronomer Schroeter, sometimes called the Herschel of Germany, near the beginning of the century. His conclusion was that the length of the day on both these planets differs but little from our own. These results kept their places in the text-books almost unchallenged until near the close of the century. The multiplicity of observers equipped with instruments greatly superior to those of Schroeter were apparently unable either to confirm or disprove his conclusions. In 1889, however, Schiaparelli found what he considered decisive evidence showing that the planet Mercury rotates on its axis in the same time required to complete a revolution about the sun, thus like our moon always keeping the same hemisphere presented to the primary body. Later Schiaparelli came to

the same conclusion with regard to Venus. The results obtained at Flagstaff, Arizona, by Messrs. Lowell and Douglas are in perfect agreement with these conclusions. Although probably correct, the question is still regarded by many as an open one. The spectroscope will give the final verdict. Attempts in this direction have already been made by two experienced spectroscopists, Keeler, at the Lick Observatory, and Belopalski, at Pulkowa. Keeler's career was closed by death, leaving this and many other important researches unfinished. Such references to Belopalski's results as have come to our notice seem to throw but little additional light on the question. It may be that the instrument has not yet reached that degree of mechanical perfection which an investigation of such delicacy demands; but we may confidently predict its ultimate success.

With regard to the rotation periods of Uranus and Neptune, we know next to nothing. Such scanty bits of information as have been obtained, however, point to short periods in both cases, 8 to 12 hours; here again we may look to the spectroscope to give us a final answer.

As to the physical condition of the planets themselves, their adaptability to the support of animal and vegetable life, we certainly know a little more now than was known during the early part of the century. The author of the 'Positive Philosophy' expresses what seems to have been the prevailing sentiment regarding the orthodox science of that day as follows: "With regard to the heavenly bodies, we may obtain practical knowledge of their geometrical and mechanical phenomena, but all physical, chemical, physiological and social researches for which our powers fit us on our own earth are out of the question in regard to the planets. The only case in which this rule may be too severe is in that of the temperature."

There was, however, no dearth of philosophers whose speculations disregarded these modest limitations. To such the planets, like our own earth, were the abode of vegetable and animal life with, of course, intelligent beings, perhaps greatly superior to ourselves at the head. As no other use in the economy of the universe could be suggested for these neighboring worlds, and as God could not be supposed to create anything in vain, the conclusion was obvious. More exact knowledge has, however, dissipated most of these plausible theories. It seems at present unlikely that a single one of the other planets, with the possible exception of Venus can now be in a condition to support the higher forms of life.

It is not now proposed to enter into a discussion as to the indications of the existence of intelligent beings on the planet Mars. Though there are some who assert with great confidence that such is without doubt the case, there are others whose opinion is of equal value who are certain that the last Eskimo was frozen to death on the planet's equator many thousands of years ago. As to the large planets, Jupiter, Saturn, Uranus and Neptune, on account of their great size and consequently slower development, it is certain that they have not yet reached a condition adapted to the support of life, unless it be in its lowest forms. There is every indication that a very high temperature exists in the case of all four of these planets, that they are largely gaseous, consisting to a great extent of vapors floating in atmospheres whose depth must be reckoned in thousands of miles, and that certainly no part of the solid nucleus is ever seen by us.

We have every reason for believing that the sun with the attendant planets, our earth included, had a common origin, that they are composed of the same materials, that the same chemical and physical laws pre-

vail throughout the system. We may feel very confident also that the same combinations of physical and chemical conditions which on this earth are associated with organic life will be similarly associated on other planets, that those conditions which prevent this development except in its lower forms above the line of perpetual snow will act in the same manner on Mars or Venus.

At the beginning of the century one comet was known to be a member of our system, more than one appearance having certainly been observed, viz., that of Halley. This comet is famous historically as the first whose return was successfully predicted, thus completely demolishing the vague and absurd notions which had been held regarding these bodies. At the close of the century something like a score have been observed at more than one appearance, one of which, that of Biele, has certainly gone to pieces, with many indications that a like fate is in store for all. Closely associated with the subject of comets is that of meteors, a subject to which the attention of all of us has been more or less directed within the past two years by the amount of space which the journals have given to the expected appearance of the November displays. This department of astronomy was quite unknown to science a hundred years ago. In the early part of the century writers who condescended to mention meteors at all spoke of them as atmospheric phenomena. As for meteoric stones, specimens of which are seen in all mineralogical collections, scientists would have none of them. Learned academicians ridiculed the idea that any one should be so absurdly credulous as to admit the possibility of a ponderous stone falling from heaven. When in 1790 an official statement signed by 300 eye witnesses of such an event was sent to the French Academy one of the distinguished physicists of that body wrote concerning it "How sad it is to see an entire municipality certifying in a formal official document to the truth of a fable which can only be regarded with pity." Finally in 1803 occurred a fall in France of so conspicuous a character and attested by such a host of credible witnesses that it could no longer be treated as a childish fable. The matter was investigated by the Academy with naturally only one possible verdict. Since then much attention has been given to this subject, but it does not appear that any part of it was shared by the minute shooting stars, with the appearance of which every one was so familiar, until the great display of 1833 had drawn attention to them. It soon began to be discovered that records of similar occurrences at various past times were to be found, and finally Professor Newton, of Yale, in 1864 brought together a series of such historic notices extending back to the year 902, October 15. It was found that these could be represented as successive recurrences of the same phenomenon at intervals of 33 years. Professor Newton, therefore, predicted with much confidence a repetition in 1866. This prediction was fully verified.

The details of the investigation, by which this was shown to be due to a swarm of meteoric bodies, of average dimensions, probably not much exceeding a grain of sand, moving in a long procession about the sun with a period of 33¼ years, we cannot enter into now. The length of the stream was found to be such that about three years were required to pass the point of intersection with the earth's orbit. In fact the particles are scattered—very thinly for the most part—over nearly the entire path.

Precisely how it came about that the display was so meager in 1899 is uncertain. Perhaps it was caused by the particles being very unequally distributed along the line and that the earth on that occa-

sion passed through a comparatively thin region. Perhaps the perturbations of the planets have changed the course of the stream to such an extent that the earth no longer encounters it. The question will doubtless receive an answer in due time.

A large number of these meteoric streams have been recognized. A list of 695 radiants, as they are called, is to be found in the *Monthly Notices*, R. A. S., for 1875. Probably, however, a considerable number of these are fictitious.

One of the most remarkable discoveries connected with this subject was announced in 1866 by Schiaparelli, viz., that the well-known August swarm of meteors moves in practically the same orbit as a bright comet seen in 1862, known as Tuttle's comet. Shortly afterwards the orbit of the November swarm was identified with that of Temple's comet of 1866. A number of other such coincidences have been found, the most remarkable being that of a swarm known as Andromedes, which appears to have taken the place formerly occupied by Biele's Comet, in short to be nothing more or less than the shattered fragments of that body. Whether, on the one hand, those streams which have not been identified with any known comet are also the remnants of such a body long since disintegrated, and, on the other, whether those comets permanently attached to our system are undergoing a like process of dissolution, we can not say with certainty, but the theory looks very plausible. One such catastrophe has been carried to completion within the memory of many now living.

Another case in which this process was rapidly developing was that of the great comet of 1882, which many of us will remember. As this body receded from the sun its nucleus was broken into seven distinct fragments which gradually separated farther and farther from each other, until the body disappeared from view. According to the best determination of its period, this comet should return in seven or eight hundred years. When it does return, if this ever happens, undoubtedly it will be in the form of at least seven distinct comets, following each other at intervals of perhaps several years. Each of these will very likely be again subdivided, the operation continuing until nothing remains but minute fragments.

We may follow the process backward as regards this body. In 1843 appeared a splendid comet whose orbit was remarkable on account of the nearness of its approach to the sun. Again in 1880 a large body of this kind appeared whose path so closely resembled that of 1843 that it was generally believed to be the same body, though how such a conspicuous object could return to our neighborhood every thirty-seven years and never have been seen before 1843 was a puzzling question. Greatly to the surprise of astronomers, the great comet of 1882 was found to follow almost precisely the same path. The theory was at once advanced that on account of the close approach of this body to the sun, passing as it did through millions of miles of the solar corona, the resistance encountered was rapidly bringing it into the sun. A few years, possibly a few months, would suffice for completing the work. What effect this collision would produce upon the sun could only be conjectured. Would it bring disaster to our earth or not?

The comet, however, pursued its way after passing the sun, with no appreciable change in the character of its orbit. It was followed from September 3, the date of its discovery, until the following June, when its distance from the sun was 470,000,000 of miles. Abundant material therefore existed for investigating its movements. The result was that at least 650 years must elapse before its return, the time being more likely to be 800 years. It

seems almost certain, therefore, that the three comets which appeared respectively in 1843, 1880 and 1882 originally constituted one gigantic body, which, on the occasion of a previous visit, perhaps about the time of the Norman conquest of England, had been torn in pieces by the sun's action precisely as was the case with the fragment which returned in 1882.

Whether the meteoric stones of which mention has been made are in any way related to the minute shooting stars, and whether or not they also at one time formed constituent parts of comets, we cannot say with certainty, but there seems to be no clear line of demarkation between the two classes of bodies. It appears to be simply a difference of dimension. A few of the fragments are massive enough to make their way through the air, and are known as meteoric stones. The great majority are so small that they are dissipated in the upper regions of the atmosphere.

On July 8, 1842, occurred a total eclipse of the sun, the line of totality passing over central and northern Europe. Great interest in this event had been aroused, largely due to the enthusiasm of the English astronomer Francis Baily. What we may call the first of the series of eclipse expeditions, since become such a conspicuous feature, were sent out at this time. Among those who made the long journey—long for those days—were the Astronomer Royal, Baily, Struve from Poulkova, Schumacher from Altona, and Arago from Paris. Though the corona and solar prominences had been frequently seen and described in a casual manner by previous witnesses of similar phenomena, such accounts had attracted little attention. Apparently most of the observers on the present occasion were totally unprepared for the spectacle which confronted them. So far as it concerns the cause of science, these now familiar appendages of the sun may be said to date their discovery from this occasion. Baily is particularly eloquent in his account of the corona, closing as follows: "Splendid and astonishing, however, as this remarkable phenomenon really was, and although it could not fail to call forth the admiration and applause of every beholder, yet I must confess that there was at the same time something in its singular and wonderful appearance that was appalling; and I can readily imagine that uncivilized nations may occasionally have become alarmed and terrified at such an object, more especially at times when the true cause of the occurrence may have been but faintly understood, and the phenomenon itself wholly unexpected.

"But the most remarkable circumstance attending the phenomenon was the appearance of three large protuberances, apparently emanating from the circumference of the moon, but evidently forming a portion of the corona. I never lost sight of them when looking in that direction, and when the first ray of light was admitted from the sun, they vanished with the corona altogether and daylight was instantaneously restored."

The importance of these phenomena was now for the first time brought home to astronomers, and the desirability of investigating their true character. A variety of theories were advanced, some old and some new, some not far from the truth and others very much so. Professor von Felitsch, of Griefwald, published a treatise in which he proved to his own satisfaction that corona, prominences and chromosphere were purely optical appearances.

Some of the mists and haze enveloping the subject were cleared away by the eclipse of 1851, which was successfully observed in Norway and Sweden, but it was not until 1860 that the true character of these phenomena, that of solar appendages, was firmly established. This occasion marked an im-

portant epoch in this class of investigations from the fact that photography was now for the first time generally applied. The photographs possessed the great advantage of freedom from personal bias and of forming a permanent record which could be studied at leisure. Although nothing was previously known of the character of the rays with which the impression must be taken, as it happened the results were eminently satisfactory. The comparison of plates taken hundreds of miles apart showed identically the same forms, thus disposing of the notion that they were due to personal or atmospheric causes, while those taken at the same place, in close succession, showed the moon to pass over them, gradually covering or uncovering them as the case might be.

The eclipse of 1868 was distinguished by another great advance in the practical application of the spectroscope. Now for the first time the true character of the so-called prominences was demonstrated, viz., that of glowing gases or vapors shooting up to heights of fifty or a hundred thousand miles above the sun's surface, and composed in great part of hydrogen. A conspicuous line was also seen near the D line of sodium. As this corresponded to no chemical element then known, it was called the helium line. In 1895 helium was discovered in a gas obtained from the mineral cleveite, an interesting case of a chemical element first discovered in the sun.

In connection with this eclipse it was found that these prominences could be observed at any time when the sun was visible by a proper use of the spectroscope. This important discovery was hit upon independently by M. Janssen and Mr. Norman Lockyer. Both discoverers communicated their methods to the French Academy, the letters reaching the Secretary within a few minutes of each other. In commemoration of this event a medal was prepared bearing the effigies of both Janssen and Lockyer.

The principle employed in obtaining the images of the prominences is as follows: The light of these objects is largely monochromatic. If such a ray is passed through a prism it is bent out of its course, losing a little of its brightness by the absorption of the glass but not otherwise. The light due to the glare of the atmosphere, however, which is the cause of our inability to see these features whenever the sun is visible, being composed of all the colors of the spectrum, is dispersed and rendered so faint as not to interfere with the image of the prominence. The higher the dispersion, the darker is the background against which this image is seen. It was at first thought necessary to employ a narrow slit, thus gradually building up the prominence by taking narrow slices in succession. It was soon found that the slit could be opened wide enough to show the entire image at once. This discovery made possible the careful and deliberate study of this feature of the sun, with the result that more is probably known of it than could ever have been ascertained, had it been necessary as at first to employ only the few moments during total eclipse.

The next step in advance would seem to be in the direction of accomplishing for the corona what had been done for the prominences and thus make possible the study of this feature of the sun's environment under the same leisurely and deliberate conditions. This problem has received a great amount of attention during the past twenty years. Various methods of attack have been suggested and tried, but so far without success.

In 1882 Professor Huggins believed that he had solved the problem. By the use of plates sensitive only to the most prominent rays of the corona, he obtained photographs which had every appearance of being genu-

ine pictures of the solar appendage. The results were regarded by many as genuine, while others doubted. Much discussion followed, some of it slightly fervid in temperature.

However, the test was simple as soon as an opportunity could be had for applying it. The eclipse of 1886 gave the wished-for opportunity. Photographs taken during totality were compared with those taken by Huggins's process, but alas! the results were far from identical. The supposed coronal forms were therefore fictitious.

Though much in the way of detail has been learned of the corona in connection with recent eclipses, it still remains very much of an enigma. Unless some new method may be found for attacking the problems which it presents, apparently their complete solution will be long delayed. Here too, as in other cases, the solution of one problem is likely to suggest a score of new ones, so that eclipse expeditions seem unlikely soon to be exclusively things of the past.

At the opening of the century, it can hardly be said that astronomers were in possession of more than two or three catalogues of stars which would be of any use whatever for the exact astronomy of today. Even these were of quite limited extent as regards the number of stars contained. There were, it is true, a number of such catalogues based upon the imperfect methods of the previous century, and a considerable amount of valuable material in the form of unreduced observations existed, but the latter was of little practical service so long as it remained in this form. Even if accessible, which was not always the case, very few could undertake the drudgery of searching through the records for the wished-for material, and when found, if found at all, to apply the reductions necessary to prepare it for practical use. It is to George Biddel Airy, who became Astronomer Royal in 1835, that astronomers owe the introduction of the present practice of reducing and publishing observations promptly, thus making them accessible to all.

The most valuable series thus buried out of sight at the beginning of the century was that of Bradley. The observations were made at Greenwich between the years 1750 and 1762. These were first rendered accessible by Bessel, who in 1818 published under the title 'Fundamenta Astronomiae,' a catalogue of 3112 stars constructed from all Bradley's observations. More recently a re-reduction has been published by Auwers, in which every refinement which the present state of science could suggest has been employed, in order to obtain from them the best possible results. This catalogue is of special value in such investigations as involve the stellar motions, the remoteness of the time of observation—140–150 years—being a great advantage in this respect. There was also a great mass of observed star places, the result of the untiring industry of LaCaille, D'Agelet and Lefrançais Lalande, nephew of the more widely celebrated astronomer. Most of this material was only placed in an accessible form after the nineteenth century was far advanced, the last contribution being the publication by our own Dr. Gould, in 1864, of the final reduction made under his direction of the observations of D'Agelet, all reduced to the epoch 1800.

The beginning of the century found Piazzi busily engaged at his observatory in Palermo accumulating material for his famous catalogue of 7646 stars, which finally appeared in 1814. As he possessed for this purpose an instrument superior to anything previously constructed, and was himself a careful and most industrious observer, this catalogue has been of very great value. A re-reduction of the observations is now in progress, based upon the more accurate

values of the constants which we now possess, and with improvements in method unknown one hundred years ago, which cannot fail to add greatly to its usefulness.

As time went on observations were conducted with more or less regularity at various places, each observer or institution acting independently of what was done elsewhere. As a result, many stars were observed over and over again, and others, equally important, not at all. In 1866, however, the Astronomische Gesellschaft of Germany organized a systematic campaign, having for its object the accurate cataloguing of all stars of the northern heavens not fainter than the 9th or 9.5th magnitude. For this purpose the entire northern heavens were divided into zones of about 5° in width, and thirteen different observatories each undertook to observe one, or in some cases two, of these zones, the work all being done on a strictly uniform plan, so that the results shall be homogeneous throughout. This work has been in progress for more than thirty years—somewhat deliberately at some places, it must be said, but is now nearly completed. The plan has since been extended to include southern stars as far as the tropic of cancer. Meanwhile our own distinguished countryman, Dr. B. A. Gould, as the result of fifteen years' labor at Cordoba, Argentina, has given us a similar catalogue of 73,160 stars between the tropic of cancer and the south pole.

The great work, instituted by the Astronomische Gesellschaft in 1865, is still unfinished, yet an even more ambitious undertaking was inaugurated fourteen years ago by an international congress assembled at Paris for that purpose. This calls for a photographic survey of the heavens to be participated in by a number of observatories—eighteen have joined in the undertaking—two sets of plates being taken. The first set are to have sufficient length of exposure to give positions of all stars not fainter than the eleventh magnitude. These are to be measured and the resulting positions catalogued. When completed, this catalogue will include between two and three million stars. The second series of plates is to have a longer exposure, sufficient to show stars of the fourteenth magnitude and will furnish charts of the heavens. 22,154 plates are called for and many years will be required for its completion. The resuits already obtained show that star positions may be obtained in this way with an accuracy little if anything inferior to the results of meridian observation.

The problem of the past history and future destiny of the solar system has occupied much attention during the century. Near its beginning Laplace had announced his famous nebular hypothesis. For many years it seems to have been taken for granted that little if anything could be added to this theory. In a general way it may be said that it forms the foundation of whatever has been developed in this direction. Laplace began with the sun already existing, surrounded with an atmosphere of heated vapor extending beyond the farthest planet. This body revolved on its axis and gradually shrunk as its heat was radiated into space. The linear velocity of the outer parts remaining constant, the angular velocity would constantly increase until in time the centrifugal force became equal to the centripetal when the central part separated, leaving the equatorial part in the form of a ring. This ring contained the material out of which the outermost planet was formed. Successive repetitions of the process produced the different planets, and these in turn produced satellites in the same manner. The rings of Saturn were held to be almost an unanswerable piece of evidence in favor of the theory. Though, without doubt, the

system was evolved in some way from a primitive nebula, we may say with certainty that it did not follow the orderly course marked out for it by Laplace. The subsequent discovery of the great principle of the conservation of energy dispensed with the original hypothesis which started with the mass in a heated condition, at the same time that it associated with it the important question as to the supply of heat and other forms of energy which are constantly being poured out with such prodigality by the sun. If we suppose the matter composing our system to have been at one time a nebulous mass, filling the present orbit of Neptune, the temperature may then have corresponded to the absolute zero so far as our purposes are concerned. The process of shrinking and condensation to the present condition would have evolved an amount of heat quite equal to that which the problem calls for, but, unless a constant supply is furnished from some source, the present process of radiation will soon come to an end. The explanation of this supply which is generally accepted was first announced by Helmholtz in 1854. He ascribed it to the shrinkage of the sun now going on. It is capable of mathematical proof that a body consisting of matter in the form of a gas, which is the case with the sun, by the process of condensation due to the pressure produced by its own attraction, will constantly grow hotter so long as it remains a gas. This operation must end when a considerable portion of its matter is reduced to a liquid or solid form. The system, then, had a beginning, and as a consequence it must come to an end. Or more properly speaking the present condition of things can not last forever. Thus Helmholtz concludes that if the intensity of radiation has been uniform from the beginning, the present order cannot have existed longer than 22,000,000 years. Others make the period less. Looking into the future, at the end of 5,000,000 years, the sun will have contracted to half its present volume, and at the end of another 5,000,000 years it will be mainly if not entirely solid, and must have ceased to be self-luminous much earlier. An interesting corollary to this subject is the principle of tidal evolution developed by Mr. G. H. Darwin. Supposing the moon to have been separated from the earth by some process at a time when the matter composing them was in a liquid condition, each body would produce enormous tides in the other. Consider those produced on the earth by the action of the moon: the effect would be on the one hand to retard the earth in its rate of rotation, and on the other to drive the moon farther from the earth. Without going further into detail, we may say that Mr. Darwin finds that if no other causes were at work not less than 50,000,000 years would be required for the evolution of the system of the earth and moon as it now exists.

It was not far from the beginning of the century that Herschel attempted the solution of the greatest of all problems—that of the structure of the universe. The problem proved, as may be supposed, quite impossible of solution by methods then available. Much has been learned during the century which was unknown to Herschel, but we seem to be as far as ever from the final solution. Instead of an orderly distribution of stars, clusters and systems, we find all apparently intermingled with vast cosmic clouds and huge dark bodies, possibly burned-out suns. For anything we know these latter may be as numerous or more so than the brilliant ones. Will the labor of another century bring order out of this seeming confusion or will it only disclose still greater complexities unknown to us? Time alone can tell.

C. L. Doolittle.

University of Pennsylvania.

*SOME UNSCIENTIFIC REFLECTIONS UPON SCIENCE.**

SCIENCE has its limitations and often confesses to them more or less directly, so that there must be some justification even from science for the heretical standpoint that I am now taking. Like all limitations, too, those of science are as much a source of danger as of opportunity to science itself. And also as for my being unscientific, it may be well to reflect that in these days any negative term can be looked upon as only an extreme or limiting degree—in one direction or the other—of that which it denies, so that I have at least an even chance of saying something scientifically worth while.

With regard to the limitations of science, it is a commonplace of the day that for accuracy and genuineness or purity science must be (1) independent of life, the subjective interests, whether personal or social, being perhaps science's most unsettling influences, (2) specialistic, the 'Jack of all trades' in science being anything but *persona grata* among scientific men, and (3) positivistic, all conceits about what is beyond actual experience and even all dogma about what seems really present to experience being most arrant heresy. But in every one of these requirements or conditions, that do indeed make science possible, there lurk serious dangers, which I wish to point out and emphasize. Not that they have never been seen or heard of before, but rather that certain things are sometimes so commonplace, so well known, as to be unappreciated, if not forgotten altogether.

So, in the first place, the ideal of objectivism for science tends, just in proportion as it is realized, to bury science in the deep grave of technique. Of course, if one believes in a resurrection, all may yet be well, but many do not or at least would blush to admit any such belief. And just what do I mean by technique? I mean everything that makes scientific work purely mechanical, for pure mechanicalism is the inevitable method of pure objectivism. Scientists have their etiquette about preempted problems or fields of research, their notions about originality as dependent on working in a new field—hence the preemption to prevent transgression or theft of originality, their conceits about bibliographical information, linguistic proficiency and technical phraseology, their satisfaction over 'publication,' 'contribution,' 'production,' and even 'research,' and a very humble deference of each to each among the different branches of scientific enquiry; and under technique I would include all these things as well as the more familiar matters of method and apparatus and material. Physicians, we are told, and not infrequently their patients, suffer from a professional ritual and etiquette, but they are far from being alone in their misery. Scientists are a close second. Of course to deny that technique has its uses would be absurd. The danger, however, not the use of it, is what now concerns us. Technique is one of the enabling conditions of science, but science that gets no further, that is only 'pure' and 'objective' and 'inductive,' is not true science; its much-vaunted observation and experiment may fill a good many pages and a good many volumes, but material, even material in books, and experiments, even carefully reported experiments, are not science necessarily.

True science, as I conceive it, and I think as all are conceiving it to-day with growing clearness, is synthetic as well as analytic, being interested in something more than a decomposable object. It is activity, not

* This paper was read in part at the Baltimore meeting of the American Psychological Association, December, 1900, and *in toto* before the Research Club of the University of Michigan, in May, 1901.

mere passive receptivity; it is invention, not mere discovery; and what so many are pleased to call the real life, subjective as this is, the real life of a person, a society, or a race, is as important to it, as much a warrant of its conclusions, as any object, however mathematically describable, with which science was ever concerned. True science, I say, is no mere knowledge of an outer world; it is invention, the invention of a tool, the making of a great machine, with use of which human life is to become more vital or more effective, more nearly adequate to the world in which man finds himself; it is what a biologist might call an instrument of adaptation to environment. Sometimes this instrument takes visible, wholly material form; sometimes it appears as method in the practical arts; sometimes it is only an atmosphere or point of view, a habit of mind; but, whatever it is, it is useful, incalculably useful, and its invention is science's chief justification.

This, objects somebody, is sentiment, and sentiment of the sort that destroys science, making serious accurate science impossible. I can not possibly agree. Is a man less interested in having a proper edge on his razor because eventually he must use it on himself? Nothing but the truth can ever set anybody free. But, all question of sentiment or of sharp razors or of a truth that liberates aside, the consistent evolutionist is obliged to take the view of science that is here asserted, just as in general he is obliged to think of consciousness as one of the positive conditions of organic development. To be an evolutionist and at the same time to think of consciousness as only an external ornament of life, perhaps a result without being a condition of development, or of science as solely for its own sake, would be nothing more nor less than to stultify one's self completely. For the historian, too, whether evolutionist or not, whose chief business is to remind us that the present is not all, the prevailing devotion to science for its own sake, which also in other times has possessed the minds and hearts of men, can be at best only a passing phenomenon. And then, apart from the standpoints of evolution and history, human society is sure sooner or later to resent what I venture to call the aristocratic temper that pure objective science is all too likely to acquire from the exclusiveness of its ritual or technique or say from its academic dress. Aristocratic temper, whatever its direction, is unquestionably as desirable in social life as it is inevitable; it is incident to the development of all institutions, political, ecclesiastical, industrial or educational; but the resentment which it is sure to awaken is not one whit less serviceable to society, insuring, as it does, among other things the 'extension' of science, the translation of science into life.

So for a time pure science may lord it over applied science, the perfecting of science as a tool being the absorbing interest, and inferior men or at least so-called inferior men may be the unfortunate representatives of science in industry and the arts generally, but in our own day applied science has begun to assume its proper place of honor, and those engaged in it are even often recognized as 'research men,' and in general the use of any tool, which men devise, with never mind how much cloistered seclusion and esthetic fervor, is as necessary as the making. The true scientist, accordingly, can only welcome enthusiastically the many indications in recent times of an offensive and defensive alliance between science and industry, seeing in these a conclusive answer to those who have raised the cry of science's bankruptcy. Furthermore, the conflict between pure science, science as technique and applied science is one in nature, and I think also in time, with that between ecclesias-

ticism and theology on the one hand and practical religion on the other and the close analogy between the two must help to emphasize the danger of the purism, which is the objectivism and technique of science, without seriously reflecting on its usefulness.

But besides burying science in technique, objectivism involves a most interesting expression of conservatism. I am not now thinking of the double truth or the double life which it sanctions so cordially that men can entertain advanced scientific doctrines without feeling them in any serious conflict with the traditional teachings of religion and morality, but something else, perhaps not wholly unrelated to this, is in my mind. Thus, while science is commonly supposed to be advanced and 'up to date,' if anything is, it is so only in a way that needs to be very carefully qualified, for it manages to perpetuate not indeed the letter, but still the spirit of old views. Certainly a purely objective science can at best only give a new material content to existing and time-worn forms of thought; it cannot do that in which progress must always consist, namely, develop and adopt new forms of thought, new categories; it cannot do this without betraying its objectivism. Objective science, for example, has said, relatively to a certain doctrine of creation, that spirit did not precede matter in time, but instead matter preceded spirit, and, except for the excitement of the drawn battle which this startling assertion has precipitated, it can hardly be said to have involved any great advance. Also, while deposing the First Cause, an objective science has made all things causes after the same plan, individual, arbitrary, antecedent causes, and this is only to multiply indefinitely, perhaps infinitely, the offensive creationalism. Not so, some one objects, since it involves a great deal more than mere multiplication, for by making all things causes it brings into science the important principle of the equation of action and reaction, a principle which, turning creationalism fatally against itself, yields the new standpoint of mechanicalism. Readily I grant this, but a purely objective science has no right to any such development; a purely objective science has no right ever to change its standpoint.

Perhaps this does not mean very much. Then let us approach the matter in another way, risking a reference to one of science's pet conceits, the familiar 'question of fact.' It has been for science a 'question of fact' whether matter made mind or mind made matter, whether this or that thing is or is not a cause of some other thing, whether certain very low, mayhap unicellular organisms, show purpose in their activities or do not, are gifted with a natural tendency to social life, a real interest in their kind, or are not so gifted, or—to take one more case—whether the changes in the brain that precede bodily movements are or are not directed by consciousness, consciousness being in the one case in causal relation with the brain and in the other only an idle external accompaniment, an 'epiphenomenon'—but in each one of these questions of fact we can see how the scientist is given to standing in his own light, obscuring the view of what he above all others ought to see. Are mind and matter, cause and effect, purpose, society, brain-process and consciousness such well-established conceptions, as if independent constants in the scientist's formulæ, that mere external questions of fact can be asked about them? Why, when one really thinks about it, to assume, as questions of fact are usually made to assume, such is their natural objectivism, that anything either is or is not something else, is about as blinding and as ill-advised as could well be. It keeps the scientist busy no doubt, eternally busy, as busy as the sportive cat that so hotly pursues her own caudal extremity, but it does

not contribute much that is positive and progressive. To the question, for example, about lower organisms showing purpose or social feeling in their activity the scientist may answer no and be quite emphatic in his answer too, but almost at once he will appreciate that mankind, when scrutinized in the same way, is similarly deficient; and then somehow the wind is taken out of his sails, since purpose and social feeling are not to be so easily disposed of, and the question of fact simply returns for another reckoning, with Shelley's cloud, silently laughing at its own cenotaph. And what is the difficulty? The difficulty is in the assumption that purpose or social feeling is a fixed conception, so fixed and so well known that its presence or absence can be established by an experiment or two on strictly objective principles. No conception is fixed, and a science that entertains a question of fact with its 'either this or not this,' 'either that or not that,' simply needs to betray its objectivism sufficiently to *recognize* that no conception is fixed, and to recognize at the same time—for this is directly implied—that any division of the things of the world into *a* and not-*a* or *b* and not-*b* is necessarily artificial. In a real universe everything must be true of everything, nothing can be what anything else is not. Let science recognize these things and it will promptly exchange its external objective question of fact for direct internal questions of meaning. Thus, for one of the cases in hand, not Are low organisms social or purposive, but What do they testify as to the real nature of society or of purpose? Being subject to the principle, which I but just now referred to and which I think is not to be gainsaid, that in a real universe everything must be true of everything, that a real universe is really indivisible, the things of man's experience, whatever they be, must always be means to man's understanding of himself, not the affairs of an wholly objective science. What they are not, he is not; what they are, he is. So, instead of denying purpose or social feeling, or even of assuming the possibility of their denial to lower organisms, science should simply seize the opportunity which its experiments afford of a clearer definition of purpose or society. Thus the experiments seem to show, not that there is no purposive activity or social life in low organisms, but that purpose itself, wherever exhibited, is only the urgency of expressing an existing adaptation, an adaptation that is at once realized and even consciously appreciated. A purely objective science could never assume the standpoint here illustrated, but a progressive science, a science for which let us say knowledge is as much a reaction as an action, a reflection as an observation, can take no other.

The conservatism of objective science or the viewpoint in its questions of fact, which the conservatism determines, is the chief reason for the negative attitude of science, so often an object of *just* complaint. Thus, to use still another illustration, for science to assume that God either is or is not, because He must either be or not be what men have thought Him, is simply to beg the theological question altogether, and true science, or at least true thinking, cannot be and should not be identified with such question-begging. Thus, for science's question of fact, a negative answer is a foregone conclusion, inasmuch as the very fact of the question is evidence that a new idea of God is only just below, if not already on the horizon of man's consciousness. What, therefore, we should ask is, not *Is God?* but simply and candidly, *What is He?* The business of science is to accept and interpret experience, not to question its very reality.

But, secondly, there is scientific specialism, a natural concomitant of objectivism, since the objective as innocent of all

subjective relations is necessarily manifold and discrete, and so turns scientific study into many separate ways. The peculiar danger of specialism is that it is almost certain to make vision dim, if not to induce complete blindness, or, as virtually the same thing, to create in consciousness curious fancies, strange distortions of reality, seen not with the eye at all, but with the mind, which is always so ingeniously constructive, so original, so imaginative, and one might even say so hypnotic in its power of suggestion over the senses. Specialism closes one's eyes and makes one dream. It makes the specialist among physicians see his special ailment in every disorder, and every disorder in his special ailment, and this so truly that merely to consult him is to fall his victim. Of course, he can never be wholly wrong, and his unwitting transgressions help discovery, but, nevertheless, his situation is full of humor. And in science generally, the specialist dreams, transgressing his own proper bounds without clearly knowing that he has transgressed. Why? Because thought, which although often apparently suppressed and abused never actually deserts experiment and observation, is so much greater than vision, than mere sensuous perception. In spite of the specialist being all eyes for his own peculiar interest, the thought that is within him, being bound to conserve an indivisible universe in every particular thing, leads him, thoughtless devotee that he is, patiently repeating his sacred syllable, into most wonderful visions, projecting his consciousness to regions of such logical subtlety and marvellous construction as was certainly never known before, unless, perhaps, among those Eastern sages who fed their minds on 'om.' A specialist, he sees the universe, not knowing in his blindness or in his dreaming that it is the universe; and his danger, the danger of all specialism, is that he may never awake.

Thus mathematics and physics and chemistry and biology and psychology, not to say also the social sciences, are dependent upon the visions of specialism. Each of them may indeed be special; but thought insists upon making its object conform to reality, which is never special, so that in each there do and must arise abstractions, logical constructions, for the others. When, for example, a physical scientist insists on seeing his world of material phenomena only physically, while in reality it is and must be a world of chemical process also, and even of vital and mental character, he is bound to admit to his thinking what he will call working hypotheses, formally true to his physical standpoint, but what any outsider, in order to explain why they are hypotheses that work, must call compensating conceptions, in short logical constructions that are substitutes for the neglected points of view. A science's working hypotheses are thus as if doors in the paneling by which the other sciences are secretly admitted to a room that seems tightly closed to all comers. Every science, in short, and this the more as the science is special and objective and exact, entertains the others unawares. Tennyson's 'flower in the crannied wall' is nothing in its all-inclusiveness when compared with a well-developed special science. In a sense that is indeed coming to be widely appreciated, no science ever does or ever can live unto itself alone. It may will to, but it does not and cannot.

But what are these 'working hypotheses' that work because they are 'compensating conceptions' or 'doors in the paneling'? Some illustration of the foregoing is now imperative. Illustration, however, is difficult, very difficult, for a reason which the scientists will allow me to mention. They know too much about the sciences, while I know too little. Still, as too much knowledge is often blinding and so is only a form

of ignorance, the situation is not altogether hopeless. Thus, while it is true that scientists are likely to insist, even in the face of the principle of thought preserving the unity of an indivisible universe in all the varied studies and conclusions of science, that physics is nevertheless only physics and chemistry only chemistry and biology only biology and psychology only psychology, and while also my illustrations must all come from the field of their special sciences and may therefore only set them more firmly in the willful blindness of specialism, still the principle itself, the principle of a conserving thought, is a disturbing influence which they cannot escape, and then besides I am for the moment forgetting and asking them to forget a very important fact of scientific study today. In these times the running together, or merging, of different sciences, as if through something of the nature of a chemical reaction, is a very familiar phenomenon, and it has been taking place with such persistence and confidence as actually to suggest a natural affinity, each of the sciences involved having the rich experience of discovering itself in the others. This fact, then, must make illustration at least less difficult, since in a way that is certain to appeal to science as no mere theory ever can, it proves or goes far towards proving what is to be illustrated. Moreover, specific illustration is hardly necessary in the sphere of the physical sciences or again in that of the social or of the psychological sciences, for within each one of these groups the affinity but just now referred to has been clearly exemplified, as in the interesting case of physics, chemistry and mathematics. Illustration, then, is needed only for the physical in relation to the social and psychological, and to this I now turn.

In articles already published under the titles 'Epistemology and Physical Science —A Fatal Parallelism,'* and 'Physical Psychology,'† I have undertaken to show, and I believe I have at least made a beginning of showing, that the dualism of mind and matter, which separates the physical and psychical sciences, is logically reproduced, as if by a sort of projection, within the special spheres of each. Physical psychology is 'concerned with the substitutes or indirections for mind [for the sort of unity, intensive instead of extensive or qualitative instead of quantitative or vital and spiritual instead of physical, which is always associated with mind] that appear in all the so-called physical sciences,' and corresponding to physical psychology there is a psychological or epistemological physics, in its turn concerned with the substitutes for quantity and matter that are present in all the psychical sciences. The sensuous self, for example, with the atomism that it has always involved psychology and epistemology in, is only as if a projection of the physical on the psychical. Sensationalism, as we all know, has ever been closely associated in history with materialism. And, on the other side, in conservation,‡ in plenitude, in motion as relative, that is to say, as always under a principle of uniformity or constancy, and also as inclining to something like vibration or rotation, in which it is an expression of rest as well as motion, and finally, not to continue what might be a long catalogue, in the infinity of space and time or—as the same thing—of quan-

*See *Philosophical Review*, July, 1898.

†See *Psychological Review*, March, 1900.

‡The case of conservation, in addition to what is to be said here and to what has been said in the two articles referred to above, may be put in this way. Thus is not the 'constant quantity,' not a mere quantity, but a ratio? As a ratio, even if finite, it is, like all ratios, more than merely quantitative, its constancy testifying, not to mere quantitative unity, but to a unity that quite transcends any purely quantitative differences. As ratios all quantities are both finite and infinite.

tity, the physical sciences have 'doors in their paneling' for the silent entrance of psychical. Do you fail to see this? Then, I can now only suggest, and probably I need only to suggest, that every one of these physical notions, either implicitly or explicitly, is paradoxical, and the paradox, whenever it arises, shows the thinker become a traitor to his chosen standpoint, to his accepted forms—in the case in hand, of course, to the forms and standpoint of physical science.* Moreover, such conceptions as conservation and plenitude and vibration and rotation and infinity, dependent as so largely they are upon what is agreeably known as the scientific imagination, are usually recognized as the physical basis of the very possibility of science, which I would now venture to define, not as mind's knowledge of matter, or in general of objective reality, but as mind's knowledge of itself in matter or in objective reality. Science, in other words, even special science, even objective science, is self-consciousness; say a very realistic self-consciousness, the self seen through the mirror of not-self; which, although metaphysical and almost offensive, reminds me and perhaps others of Burns:

> "O wad some power the giftie gie us,
> To see oursels as others see us!
> It wad frae monie a blunder free us,
> And foolish notion."

The bonnie Robert was too much of a specialist in poetry to see that science was the very thing he prayed for.

But now for further illustration of the way in which thought defies specialism and conserves its universe I would mention several important facts, that are certainly not unfamiliar, as follows: Thus the social sciences imply affinity for the physical sciences, in that, besides their more special divisions, they are constantly making appeal to science in all its branches from physics to psychology; and the biological sciences, in addition to their more conventional forms, are becoming most hospitable to psychology, chemistry, and even to mechanics. Again, all the different sciences, however special, are wont to adopt the same general method, as, for example, the historical method, the consequences of which to the cause of pure specialism may easily be inferred. And, lastly, striking analogies, other than that of method, are always easily traceable among the sciences of any particular time. Atomism in physics is contemporary with individualism—consider Democritus and the Sophists—in politics; a monarchical politics with an anthropomorphic creationalistic theology and a heliocentric astronomy; and a Newtonian astronomy, which makes a law or force instead of an individual body the real center of the solar system, with democracy or constitutionalism and inductive instead of deductive logic and naturalistic theology; so that at no time, whatever a scientist's interest, can he fail to have at least a formal sympathy with other sciences. Analogies among the sciences, so often recognized in these times, are not exactly 'doors in the paneling,' but they may be said to make the paneled partitions all but unsubstantial and transparent.

Specialism, then, is more formal than real. The special science needs only to develop to become, and to find itself, universal. The barriers with which it surrounds itself gradually vanish into mere imaginary lines, which only long usage can possibly make seem substantial and opaque, so that specialism by a logic of its own or by

* Thus conservation as quantitative is a paradox since the constant quantity cannot be finite and infinity is not a mere quantity. The plenal medium can be material only if displaced by material things and plenal only if not material and it is used, too, as an immobile, albeit elastic to an infinitesimal sensitiveness, basis of motion. Motion itself is also rest. And infinity, as already implied, is a quantitative paradox, which means a 'door' for quality, for the intensive unity of mind.

the logic of a thought that conserves its universe even in the varied studies and conclusions of the many sciences, is destined to end in the unification of the sciences. To say the very least it is the natural fate of the special science to develop into methods of each other.

Unification of the sciences, however, implying as it does the decline of specialism and so also the decline of objectivism, brings with it the translation of science into life, in short the application of science, of which, in addition to what was said before, I would now speak again, but briefly and concisely, for the positivism of science is still to be considered.

The decline of specialism, which we have found to be natural to specialism, by making the dividing partitions unreal is bound in the first place to free the sciences from that bondage of technique, just as, for example, the decline of religious—or irreligious?—sectarianism, a form of specialism certainly, is bound to free religion from the bondage of ritual. Secondly, it must make the distinction between self and not-self, subject and object, man and nature, only a formal one, formal in the way in which the special sciences themselves are distinguished, since the unity of the objective world is one and the same with the self or subject. This we but just now saw, when we were able to define science as not mere knowledge of an outer world, but self-consciousness, realistic self-consciousness, to understand which only reflect further upon the art and literature so natural to an age of science. Art and literature are self-consciousness. But, thirdly, whether because of the freedom from technique or because the scientist does come to discover his own image in the clearing and quieting waters of science, the decline of specialism, again like the decline of sectarianism, brings what some are pleased to style the liberation of the human spirit, a liberation that means freedom in, not freedom from, the natural world, and what a psychologist would call the development of knowledge into will, in short the application of science. Of course applied science must be not special, but general, because life is general; not ritualistic, but spiritual, because life is not ritualistic, making no fast distinctions between part and part or part and whole; and practically or even intuitively wise or skilled, as well as confidently volitional, not technically learned and esthetically satisfied, because life is not learned, but wise. Yes, the natural decline of specialism means the unification which is also the application of science, and, to bring the matter home, any scientific association, through which the sciences find each other out is really dangerous to the cause of pure, of objective and special science, since it can only forerun the movement of science into life.

But now as to positivism, at once the third condition and third danger of science. It can hardly be necessary to show that this is involved alike in objectivism and in specialism. Positivism confines knowledge to actual experience and to only a tentative confidence in actual experience. Scientific knowledge is positivistic, because it is obscured or refracted by the aloofness of the scientific point of view. Science is aloof from life and—in its specialism—also from itself. Then, when men who would be scientists withdraw, as we say, from affairs, it is as if they had put on distorting and discoloring glasses, through which they would see the world, the 'objective' world. The space and the time, for example, in which they see things are widely different from the space and time in which things are doing, from the space and the time of will and action. The difference is felt by us even in ordinary life, but the extreme attitude of science greatly exaggerates it. For science space and time are quantitative, divisible,

formal, independent of what is in them; for will and action they are qualitative, indivisible, inseparable from their content. Again, the scientists reduce causation to mere uniformity of coexistences or sequences, which is no real causation at all, being only so much passive existence or fatal process; while will or action is causation, the positive interaction of things that are, the active relation and conservation of what was and is and will be. And, once more, science needs elements, while will or life is the eternal denial of elements or anything like them. Says a recent writer:* "It is one of the greatest dangers of our time that the naturalistic (or scientific) point of view, which decomposes the world into elements for the purpose of causal connection, interferes with the volitional point of view of the real life, which can deal only with values and not with elements." Of the danger involved I shall speak in a moment,† but the bondage of science to elements, to thoroughly decomposed reality, is indubitable. And then, in addition to the formal space and time, the empty causality and the unreal elements, that are peculiar to the aloofness of science from life, there are in the special sciences the different 'working hypotheses,' which we found to serve the purposes of protecting conservation against specialism, but which, nevertheless, so long as retaining their projected forms, make science artificial. Science, accordingly, has no choice; it is condemned to positivism. Even the much-vaunted experience of observation and experiment, although our only possible source of knowledge, can never lead to direct knowledge of reality, can never put us face to face with that which is. Even in science we know appearances, not things.

But what now is the danger? The writer quoted above says it is the interference of the scientific with the volitional point of view. With not less truth, however, it is that the two points of view will not interfere, that both science and life will fail to appreciate, as that writer has failed to appreciate, the true import of their incongruity, and so will forever stay apart, the one losing itself in a morbid intellectualism, the other in a dead monotony of mere existence. Whatever be true about their incongruity, life without science is certainly lifeless; science without life, meaningless —as meaningless, as empty, as the proverbial Greek. We know men who lead what we often abusively call the double life. They have their science, perhaps their laboratories and their books and their own pet doctrines, and they have also their social affiliations in business, in politics and in religion; and their life seems double, because their sociology and their business, or their political theory and their party ties, or their biology and their religion, simply will not mix. But is their duplicity as real as it seems? To them, as well as for us looking on, the opposition needs only to grow to make all the science meaningless and all the life dead; certainly a strange, ineffectual opposition; a double life, that can be double only in form, only numerically and that must be tedious and unhappy even in its peacefulness. And what more can be said? This. Such duplicity, the duplicity of science and life as never interfering, is not even possible. *Of course* scientific technique, with its aloofness and its logical constructions, and life that in its special affairs is only conventional and ritualistic, or say, routine in the study or the laboratory and routine in the church or the market place, can never conflict, but routine is never either real science or real life. Witness the avowed, although sometimes forgotten, positivism of technical

*See Münsterberg's 'Psychology and Life,' p. 267. Houghton, Mifflin & Co. 1899.

†See also Münsterberg's 'Grundzüge der Psychologie' in the *Psychological Review*, May, 1901.

science and the unrest, the bravado that is so ill-disguised, of what some call 'life.' Science knows that it does not know, that it can not know, that even conscious man has always moving within him another relation to his world than that of knowledge; and life, as apart from knowledge, shows that it does not live. So again I say that the real danger of positivism, of a blind or forgetful positivism, is that the naturalistic or scientific point of view and the volitional point of view will not interfere with each other. Certainly within the laboratory and the study to keep them apart, to separate theory and experiment, would be fatal to both; the life that we call science needs their constant interference, and with every one of its experiments shows that they are not as incongruous as they seem; but what is the world, if not a great laboratory that is related to the smaller as real life to the theater, as nature to the conservatory, as an unaided vision to the microscope?

Agnosticism is another name for positivism. The positivist, the devotee of pure, objective, special science, cannot but believe in an unknowable, and this belief, in its turn often forgotten, needs always to be recognized as a part, a very important part, of the scientific consciousness, for it is only one other way in which thought conserves its universe. Thus the unknowable, whether seen as compensating for science's aloofness from life or for the dreaming that specialism induces, is a constant safeguard against the abuse of knowledge.

The unknowable is a negative that bears constant witness, not to another sphere which some mind quite different from our own might consciously comprehend, and which we, being intellectually outside, and so only creatures of faith, can merely blindly will, but to another relation than that of mere knowledge, which we *as knowing creatures* have to reality. There is, in short, an unknowable for the single reason that to know is also to will. Or, again, the unknowable is not for knowledge, but for action.

Let us be blindly scientific, insisting on science being only for science's sake, recognizing nothing as worth while but great learning about a Greek particle or a minute insect or a mysterious element, and like a dark cloud there arises and spreads over our view the unknowable, and from this cloud a voice comes: "Only the All is and the All is One and the One is not for knowledge." But as we apply our science, breaking through the walls of specialism, and liberating the will that was for the time their not unwilling prisoner, the sky clears. The one is not for knowledge, but for life; knowledge is not for knowledge, but for will, its natural fulfillment. "The end of man is action, not thought, though it were the noblest."

Alfred H. Lloyd.

TRUMAN HENRY SAFFORD.

A long, active, busy life, devoted without reserve to teaching, to research, to cares of family—such a life of science as that which closed on June 12, in Newark, cannot receive adequate appreciation in the brief space available here. But the friendship of years crowding one upon another will not let pass in silence the death of Truman Henry Safford. A few words of personal sorrow demand immediate expression; leaving a more complete summary of his life's tribute to astronomy to await dignified publication in the annals of those learned societies of which he was a distinguished member.

The friendship of years is no light thing. It was in the latter part of 1884 that Safford paid his first visit to the modest observatory of Columbia College, then situated in 49th Street, N. Y. He found there a stripling engaged in testing a level. The

youngster noticed a kindly face appearing in the doorway; conversation began about the level—and from that day on, no year has passed without the interchange of friendly visits between Safford and the writer of these words. Often and often have we sat far into the night in his 'den' at Williamstown, our talk always of astronomy and its masters. And well he knew the masters, ever recommending and emphasizing that they be studied at first-hand, not through editorially emasculated translations and editions. Gauss was an especial favorite; for Safford was first of all a teacher, and believed that mathematical instruction should always include concrete examples, especially numerical ones. The 'Theoria Motus,' with its endless ramifications of trigonometrical applications and its orbit computations, was beloved by him both for personal reading and as a text for his students. Bessel's works, even those less frequently read, like the 'Tabulæ Regiomontanæ,' he had at his fingers' ends. Text-books attracted him less. He did not use Chauvenet's astronomy, that *vade mecum* of the younger generation; it is doubtful if he owned a copy.

Indeed, a rich fund of anecdote might be collected to illustrate Safford's lovable quaintness of character. One of the cherished dreams of his life was his plan of an extended visit to Europe, its astronomers, observatories and places of historic or scenic interest. Circumstances always prevented this voyage; yet, though he was never abroad, he possessed a most intimate acquaintance with foreign countries. Few Londoners could equal his knowledge of the geography of their city. The interminable intricacies of its streets he had studied from maps until, as his friends said, he could have found his way anywhere without a guide. 'Bradshaw,' the incomprehensible British railway time-table, was an unsealed book to him. A copy was always close at hand in his library at Williamstown, and he was never tired of extracting from it new and difficult railway problems with their solutions, to the huge amusement of his family and friends.

This ungratified longing for foreign travel showed itself in still another amusing way. On the occasions of his visits to New York, he would often go to parts of the city frequented by foreigners, and afterwards entertain his friends with odd experiences, especially in the foreign restaurants. On one occasion, in the year of the Chicago Exposition, it fell to Safford to act for a day as *cicerone* in New York to two distinguished Englishmen, one a professor, the other an admiral. All three returned from their day delighted; it had been passed at an island much liked by excursionists. Here Safford had discovered, to his great glee, a place called *Klein Deutschland*, and could imagine himself for the moment in Germany. Afterwards, it always gave him indescribable enjoyment to talk of this trip, and especially how acquaintance had been made with a man who insisted on explaining the men-of-war lying in the Brooklyn Navy Yard, and was even led on to go a little into astronomy. Neither the two professors nor the admiral made known to their informant that they had even so much as heard of stars or ships before. Safford had an inexhaustible love for the humorous and the quaint; to those who knew how to understand him, he could be most entertaining. He was passionately fond of music, and could appreciate the best.

But we must not give too much space to those characteristics which attracted his friends so strongly, to the neglect of that which the world of science owes him. Born January 6, 1836, at Royalton, Vt., he showed already in his early boyhood the extraordinary arithmetical powers which distinguished him through life; for he could

at all times multiply mentally very large numbers, knew off-hand the multiplication table to 1,000, and most of the logarithmic tables to three figures. At fourteen he is said to have calculated a cometary orbit, and he graduated from Harvard at eighteen. For some years he was observer at that university under Bond, and for a short time was acting director. In 1865 he took charge of the Chicago Observatory, where he began as his serious work the observation of one of the *Astronomische Gesellschaft* zones. But his work was cut short and his position lost through the great Chicago fire. He then entered Wheeler's astronomical survey in the far West, and worked during several years for the government scientific bureaus of Washington. It was not until 1876 that he finally settled down for life as professor of astronomy in Williams College, Williamstown, Mass.

Here was done his principal work, which related especially to star positions and star catalogues. He made an elaborate discussion of all existing observations of the stars most suitable for determining geographical latitudes in the United States. This resulted in a catalogue of 2018 stars, which was published by the Engineer's Department, U. S. Army. Later, he made a similar catalogue of 612 stars, and upon it has been based the new boundary between the United States and Mexico. This was published in the report of the Mexican Boundary Commission, Washington, 1898. Safford built at Williamstown a meridian observatory which is a model of its class. In it he installed a Repsold circle, with which he made extensive observations of the close polar stars. He liked these stars especially, and the unusually lengthy numerical calculations connected with them did not frighten him. He needed no observing list, as his memory never failed to give him the instrumental setting for each of his beloved polars in every possible position of his instrument. These polar observations were collected and published by him in the 'Williams College Catalogue of North Polar Stars.'

But all this work belongs to a class important to pure science, though comparatively uninteresting to the general public; for Safford possessed especially that kind of devotion which can give generously to science without hope of public notice. More striking, perhaps, was his confident prediction in 1861 of the minute unseen companion of the bright star Sirius. Basing laborious calculations on the tiny irregularities in existing observations, he was able to show just where the little *comes* must be. And there it was found in January, 1862, by Alvan Clark, of Cambridgeport, Mass., while he was testing the 18-inch glass now mounted at Evanston, Ill.

Safford was a frequent contributor to astronomical and educational publications, and a member of many learned bodies. The Royal Astronomical Society, of London, honored him with an election as associate in 1866, when he was but thirty years of age. Great as were his abilities in astronomy, he was yet at his best as a teacher. Those who came under his influence at Williamstown can bear witness to this; his ablest pupils profited most from his stored learning, and some from among them are laboring for astronomy today. He was a loving and dearly loved husband and father. To him were granted these priceless blessings: a devoted wife, a united family, a few time-tested friends. A man of genuine piety, the conflict of science and religion had no terrors for him. He knew that no such conflict exists; that the foundations of belief rest not upon ponderous tomes of logic, but on simple faith. Such faith he had, as of a little child; and, like a child asleep, so shall God's acre rest him.

Harold Jacoby.

SCIENTIFIC BOOKS.

A Synopsis of the Mammals of North America and the Adjacent Seas. By DANIEL GIRAUD ELLIOT. Publications of the Field Columbian Museum, Zoological Series, Vol. II. Chicago, Field Columbian Museum; New York City, F. P. Harper; London, R. H. Porter; Berlin, R. Friedländer & Son; Paris, J. B. Bailliere & Son. March, 1901. Pp. xv + 471. Pls. XLIX. Text figs. 94. Price, $3.00.

Until the appearance of Mr. Elliot's synopsis, no general work on the mammals of North America had been written since 1857, the date of publication by Baird of the eighth volume of the Pacific Railroad Survey reports. The need for a work of this kind may be appreciated from the fact that the number of mammals known to inhabit America north of Mexico has increased, during the past forty-four years, from about 300 to over 1,000.

"This Synopsis," the author writes, "is an attempt to bring together all the forms of North American * mammals that have been described, and which are generally considered as entitled to some kind of recognition. * * * The present time cannot be supposed as opportune for a final and satisfactory revision of the various groups * * *. That must be the work of some future Mammalogist, who can bring to the task not only a thoroughly unprejudiced mind, but who may have acquired a more intimate acquaintance with the quadrupeds of those sections of our country, as yet little known, and whose knowledge of geographical distribution of mammals, the extent of the individual variation of crania, the relationships that apparently different forms have for each other, and the changes in color assumed by the pelage throughout the year, and in some cases adopted by the sexes, has been gained from extensive series of specimens, much greater and more complete than those possessed by any naturalist at the present day. * * * This Synopsis, therefore, may only be regarded as a starting-point upon which such a final list may be founded, and does not purport to indicate how many species of mammals there are in North America, but merely to show how many forms are given some kind of distinctive rank at the present time * * *." Obviously the work is intended as a reflection of current opinion, and not as a critical treatment of the subject. Therefore it will be necessary, in order to appreciate the book, to gain some understanding of the tendencies of the more recent work on North American mammals, and also of Mr. Elliot's attitude toward the conclusions that he wishes to summarize.

* Or, more correctly, species inhabiting the United States and Canada, since those confined to other parts of the continent are omitted.

The object of systematic biology was defined by Cope as the accurate statement of the results of organic evolution. Convenience in naming and labeling individual specimens is no longer to be regarded as of more than secondary importance. During the past fifteen years, the period of greatest activity in the study of North American mammals, series of specimens aggregating many thousands of individuals have been collected with the special object of showing the results attained by the process of evolution at the present time. These results, though not yet thoroughly understood, have been found much more complex than had been supposed. Species and subspecies, to use for the present these obsolete terms in the absence of the single word needed to replace them, are not invariably separated from their allies by characters easy to describe. They are interrelated with every degree of intricacy, and it is through the study of the more closely allied forms that the most significant facts are to be discovered. Sharply defined species have lost their history and with it their chief interest. No systematic work, therefore, is more important than the record of minute differences; and such difficulties as are met arise from the nature of the results which the process of evolution has reached, and from the inadequacy of language to express all that can be detected by the eye. That these difficulties are not averted when ignored is the standpoint of those with whose work Mr. Elliot deals. His attitude toward it may be learned from the second paragraph of the preface:

"It is very manifest to many Naturalists that too many forms have been given distinctive rank, and without doubt a considerable number of the so-called species and subspecies contained in this volume will eventually swell the

list of synonyms, already sufficiently formidable. In late years there has been an evident inclination among some Mammalogists to unduly magnify, as it would seem, trivial dissimilarities observed among their specimens, and thus greatly increase the number of slightly differentiated individuals elevated to a separate rank, at the risk of reducing the science to one founded on labels and localities, instead of distinctive and prominent characters, and thus a knowledge of the place where an example was obtained becomes at times of more importance for its identification than are the differences that may separate it from its allies. The lack of resemblances often observed among crania is frequently but the individual variations of a type, and taking these for characters upon which to establish a new species is apt to lead to error, and in not a few instances too much reliance has been placed on such slight differences. The same may be said of shades of color, and not a few names in this Synopsis have been given to specimens so closely alike, that one author, in speaking of his key, which was intended to be the means for distinguishiug the species, has been obliged to say: It will be necessary to have both skins and skulls in hand, and even then *it will be impossible to identify some of the forms without actual comparison with their nearest allies*. The scientific value of such species (?) can only be very questionable at the best, and the elevation to a separate distinctive rank of such intimately related creatures cannot be considered as helpful or beneficial to Mammalogical Science. It can be safely asserted that there is hardly a genus of North American Mammals that does not contain too many named forms, and that the science would be benefited if a considerable number were relegated to their proper place among the synonyms."

No more would be required to show that the author is not in sympathy with the tendency toward minute accuracy characteristic of the work of to-day; but this hostile attitude is so insistently brought before the reader that one more allusion to it may be made. At the end of the account of the prairie wolves, rendered exceedingly obscure by the omission of details concerning skulls and teeth (pp. 301–303) occurs the following footnote: "It is difficult to distinguish these varieties of the coyote by any description, and still more so by the skins or skulls."

It is to be regretted that an author who is admittedly unable to present his own views throughout, and who is so obviously at variance with the opinions of others, should not have been content with bringing together as impersonally as possible the multitudinous scattered papers that now make the literature of the subject a maze. In the work as it stands, personal opinion, confessedly defective, and compilation, often not free from bias, are so blended that neither is convincing; and considerable knowledge on the part of the reader is required to avoid misunderstanding.

The book is well printed, though on glazed paper that is probably ill adapted to withstand constant use. This, however, was rendered necessary by the profuse half-tone figures with which the text is illustrated. Throughout the work a tendency to abbreviation is manifested, which, while it may have curtailed the length of the volume by a few dozen pages, has not increased the usefulness of the book. The time saved to author and printer by the use of such references as Schleg. Abte. Geb., or S. S. S. does not compensate the reader for that lost in search for the meaning so effectually concealed. Probably the feature of the work which will most forcibly impress those by whom it is most needed, and that which may be said to be its great defect, is the absence of keys. Of these there are none, either to species, genera or higher groups. The diagnoses of the 10 orders, 34 families, 115 genera and 998 species and subspecies occupy about one-third of a page each. In those relating to species and subspecies there is a noticeable lack of detail concerning skulls and teeth; while the characters given are often compiled in such a manner as to be misleading. The classification is essentially that of Trouessart, with the order reversed to begin with the marsupials. Species and subspecies are arranged in accordance with their supposed affinities, and, as might have been anticipated, the result is frequently less satisfactory than a purely alphabetic sequence would have been. No allusion is made to habits or life histories, and no English names are used except in the table

of contents. The technical nomenclature departs little from accepted usage, though it may be noted that the name *Cabassous* McMurtrie, 1831, is substituted for *Tatu* Blumenbach, 1799, *Manatus* Storr, 1780, for *Trichechus* Linnæus, 1758 (as applied to the manatee), and *Dicotyles* Cuvier, 1817, for *Tayassu* Fischer, 1814. The name *Trichechus*, though based on the manatee alone, is applied to the walrus. *Aplodontia* is altered to *Haplodontia* to conform with rules irrelevant to nomenclature, but *Reithrodontomys*, *Cynomyonax*, *tridecemlineatus* and doubtless many others, which on the same ground might be changed, are left in their correct original form. A single new name is proposed, *Balænoptera velifera copei* (page 13). As if to compensate for the brevity of the text, the book is illustrated with a profuseness hitherto unknown in similar works. The plates and text figures contain half-tone reproductions of photographs of the skull and teeth of at least one representative of nearly every genus and subgenus. The standard of excellence of these figures is very high, and no equally successful application of photography to zoological illustration on so extensive a scale has hitherto been made.

GERRIT S. MILLER, JR.

THE MARYLAND EOCENE BOOK.

THOSE who have had to deal with official geologic surveys know how frequently it is argued that publications of a character so purely scientific as paleontology appeal to a very limited audience, too limited, it is often urged, to justify the expenditure necessary for their proper production. The counter-argument, however, gets at the facts. Toward the close of the Second Geological Survey of Pennsylvania, after a cloud of volumes upon tectonics, economics and devitalized stratigraphy had been issued, Dr. Lesley prepared a 'Dictionary of Fossils,' profusely illustrated, suffused with important facts, amusing fancies and pages of errata, but a work of great usefulness. The demand created by the appearance of this book was so much greater than the supply that the edition was gone while its reportorial predecessors were resting on the shelves waiting for inquirers. In New York, Ohio and Illinois the experience has been very much the same.

'Maryland Geological Survey: Eocene' is the title of the first of a series of monographs upon the historical geology of Maryland, that is to say, its stratigraphy and fossils, in other words its paleontology. It seems eminently appropriate that the Eocene is chosen as the subject for the initial volume of this series in view of Professor Clark's exhaustive acquaintance with this formation. The state geologist, though taking a leading part in the composition and execution of the work has brought into cooperation with himself a number of experts in various special lines of research. Thus the leading chapter on Eocene stratigraphy and sequence, 'The Eocene Deposits of Maryland,' is by W. B. Clark and G. C. Martin. Under the part assigned to descriptive paleontology the chapters are as follows: Reptilia, by Case; Pisces, by Eastman; Arthropoda, and Bryozoa, by Ulrich; Mollusca, Brachiopoda and Echinodermata, by Clark and Martin; Cœlenterata, by Vaughan; Protozoa, by Bagg, and Plantæ, by Hollick. Ten plates of maps and half-tones illustrate the first part of the work, and fifty-four process plates, largely of McConnell's fine pen-and-ink drawings, are devoted to the fossils. Like the other volumes of Professor Clark's survey this is exquisitely constructed on finished lines and commends itself to lovers of well-made books, who will all pray that the supercalendered wood-paper used for the plates may last until the world has no further need of the facts registered upon them.

It is an admirable achievement, bringing together in one place all that is now known of the Eocene stratigraphy and fauna in an important political division, and long after the present quarry workings, coal mines and clay pits of Maryland shall have been exhausted and abandoned, and all the economic products of the State shall have been converted into cash, the facts here brought together will endure, with a never-lessening value to the sum of human knowledge. The book will receive a welcome from paleontologists and merits the cordial appreciation of the citizens and students of the State for whom it has been specially prepared.

JOHN M. CLARKE.

SOCIETIES AND ACADEMIES.

ZOOLOGICAL CLUB, UNIVERSITY OF CHICAGO, WINTER QUARTER, 1901.

II.

At the meeting of February 6 the program consisted of a paper by Mr. C. M. Child, entitled 'Regulation in *Stenostomum*.'

This paper dealt chiefly with some of the phenomena of regulation following the cutting of an asexual chain of *Stenostomum*-zooids at various points. Some of the more important points are as follows:

The changes which occur after cutting a chain of *Stenostomum*-zooids differ greatly according to the position of the cut with respect to the zones of fission and according to the stage of development of the latter.

Pieces above a certain relative size, cut within the limits of a single zooid, regenerate the anterior and posterior ends without the formation of a distinct bud of new tissue, although proliferation occurs at the cut surfaces. A very rapid change of form occurs, the piece becoming longer and more slender and acquiring the 'normal' form of a *Stenostomum* individual. The time required for the completion of the regulation varies with the temperature, but is in all cases very short. At a temperature of 75° F. it is only three or four days.

If the piece consists of parts of two or more zooids, *i. e.*, if it contains one or more zones of fission, the processes of regulation are more complex. In pieces consisting of the posterior portion of one zooid and the anterior portion of the succeeding zooid the changes depend upon the stage of development of the organs about the zone of fission. If the zone of fission be at a very early stage it may disappear entirely, the whole piece becoming a single individual with head and tail at the two ends. In this case the part which was destined to become the posterior half of an individual becomes, in consequence of its separation from the chain, the anterior half, while the portion behind the zone of fission, which was destined to become the anterior half of an individual, now becomes the posterior half.

If the zone of fission is older, *i. e.*, if the new brain behind it is well established, all that portion of the piece which lies in front of the zones of fission is absorbed by the part posterior to it, *i. e.*, the part which possesses the brain. This occurs even though the anterior piece is much the larger. As the anterior piece is absorbed it is reduced in size, and the zone of fission and the brain appear to migrate in the anterior direction until finally they reach the anterior end of the whole piece, *i. e.*, until the anterior part is completely absorbed. In a recent paper (*Proc. Cal. Acad. of Sciences*, Ser. III., Zoology, Vol. I., No. 6, 1901) Ritter and Congdon have asserted that the brain actually does migrate in pieces of this kind. This seems to be an error of interpretation. Continuous observation of the pieces shows that the facts are as stated above. Indeed, the migration of the intestine out of the anterior piece can be observed.

If the zone of fission has reached a late stage of development, so that the brain of the posterior zooid is well formed and the pieces are more nearly ready to separate, little or no absorption of the anterior piece occurs and the two pieces separate as they would under normal conditions, the anterior pieces developing a new head and the posterior piece a new tail at the respective cut surfaces.

The absorption, when it occurs, is always in the posterior direction, *i. e.*, the part deprived of a brain is absorbed by the part which still retains the brain. The actual disappearance of the zone of fission occurs only in the early stage when the brain posterior to it is not developed or is at a very early stage of development. In general the results depend upon the presence or absence, or the relative development, of the brains in the parts of zooids which make up the pieces.

In sexual individuals of *Stenostomum* asexual multiplication ceases, though the single individual may attain a length as great as a chain of eight or more sexual zooids. The power of regeneration in sexual individuals is not as great as in the asexual chains. The energy of the body appears to be directed in large part toward the elaboration of the sexual products, the consequence of this condition being the cessation of asexual multiplication and the reduction of the power of regeneration.

The session of the Club held on February

20 was devoted to an account by Mr. E. H. Harper of 'Regeneration in *Nais lacustris*.' The paper comprised the results of experiments on both sexual and asexual individuals of this species. The following is a brief abstract:

In the asexual forms where from one to five anterior segments are removed the same number is regenerated as was cut off. In this region in the sexual forms the head segment alone is regenerated, or frequently the surface heals over without regenerating.

When more than five segments are removed in the asexual forms a pharyngeal region of five segments is regenerated. In the sexual animals the process is much slower and in an advanced sexual stage a dwarfed region of five very short segments is produced. The clitellum and sexual organs may be regenerated. This has been seen to occur after a considerable time.

So far as could be determined mutilation never causes the disappearance of the sexual organs, but the formation of a regenerating region will under certain conditions inhibit the process of asexual multiplication and cause the disappearance of the zone of fission. This effect may be produced by a cut anterior to the zone of fission, less often by a cut posterior to it, and occurs only when the zone is embryonic. The zone is also more likely to disappear if the cut is near to it. The band of transparent embryonic tissue redifferentiates and the energy of growth is transferred to the regenerating region.

In the asexual individuals the power of regeneration is less in the anterior specialized region and also in the posterior budding region than in the middle of the body. Short posterior pieces may live a considerable time, but do not regenerate. Anterior pieces of eight or nine segments may survive a short time without regenerating, but pieces of the same length from the middle of the body regenerate freely.

Internal conditions favorable to proliferation, such as the exposure of cut surfaces of intestine and blood vessels, are present in nearly all possible experiments. But if a corner of the head segment be removed, including the prostomium, without injuring the pharynx, the ectoderm will close over the surface and regeneration will not take place. In the sexual forms failure to regenerate may occur when the pharynx is cut through. Here the tendency to proliferate is slight and it may be that the epithelium differentiates completely over the cut surface before any appreciable amount of regeneration has occurred. And it is probable that after the epithelium is formed there is no further tendency to regenerate.

Growth takes place at right angles to a cut surface; and if the cut is oblique the bud will grow out at an angle to the axis of the body. Straightening is effected after the penetration of the lumen of the intestine into the region under the influence of its peristaltic motions.

Many of the internal phenomena of regeneration may be observed in the living animal on account of the transparency of the body wall. Migration of pigmented cells from the intestine toward a regenerating region can be seen. If a corner of the head segment be cut off, including one eye, the pigmented portion of the other eye is usually fragmented and pigment migrates in various directions.

It has been stated that the regenerative power of these animals varies with the season of the year, being less in autumn and winter. No evidence of this has been observed, but the evidence points rather, as we have seen, to an inverse correlation between regenerative power and sexual activity. Budding, *i. e.*, asexual, individuals regenerate freely and completely in December. The sexual forms are found from October to December.

The program for the meeting of March 6 consisted of two papers, 'The Excretory System of the Bryozoa,' by Miss A. W. Wilcox, and 'Montgomery on the Spermatogenesis of *Peripatopsis*,' by Mr. E. R. Downing.

Miss Wilcox reviewed the literature upon the excretory organs of the Bryozoa and gave the results of some work of her own on the Phylactolæmata. Her paper will appear in full elsewhere.

Mr. Downing's paper consisted of a review and discussion of Montgomery's recent study of the spermatogenesis of *Peripatopsis*.

C. M. CHILD.

THE NEW YORK SECTION OF THE AMERICAN CHEMICAL SOCIETY.

THE last meeting of the season of the New York Section of the American Chemical Society was held on Friday evening, June 7, at the Chemists' Club, 108 West 55th Street. Dr. Charles A. Doremus occupied the chair.

The chairman delivered his annual address on 'The Development of an American School of Chemistry,' in which he urged the advancement of chemical engineering by the development of originality and the assumption of greater responsibilities by chemists.

The secretary's report showed a net gain in membership of sixty during the year, and that twenty-nine papers had been read at the nine meetings which were held.

The election of officers for 1901–1902 resulted as follows:

Chairman—Professor Marston T. Bogert (Columbia University).

Vice-chairman—Durand Woodman (127 Pearl Street, N. Y.).

Secretary-treasurer—Professor J. A. Mathews (Columbia University).

Executive Committee—P. C. McIlhiney (Columbia), Professor E. H. Miller (Columbia), T. C. Stearns (Jersey City).

Delegates to the Scientific Alliance: Wm. McMurtrie, Professor Marston T. Bogert, H. C. Sherman (Columbia University).

Papers were read as follows:

M. T. Bogert and L. Boroschek—'Some Experiments with the Mono-nitro-orthophthalic Acids.'

H. C. Sherman, J. L. Danziger, L. Kohnstamm—'On the Maumené Tests for Oils.'

E. F. Kern—'On the Separation and Determination of Uranium.'

The paper on the 'Maumené Test for Oils' was a brief account of the principal results obtained in a series of experiments on several varieties of oils, with different modifications of the Maumené test. The common practice of diluting oils with petroleum to prevent too violent a reaction was found to give unsatisfactory results, the figures obtained from such mixtures being too high; as was also the case when the oil was dissolved in an equal weight of carbon bisulphide or chloroform. The necessity of taking account of the specific heats of the oils and diluents was noted. In order to avoid the necessity of diluting the oil and the resulting uncertainty in the interpretation of results, the use of a weaker acid was proposed. Sulphuric acid of about 87 per cent. can be added directly to all the common oils and the test can be carried out in exactly the same way for the drying as for the non-drying oils. Even when calculated as 'specific temperature reaction' the results are somewhat influenced by the strength of the acid used, higher figures being obtained with the more concentrated acids. It was, therefore, recommended that the test be always made with acid of such strength as will give with water a rise of 33° to 34° C.

It was announced that the courtesies of the society had been extended to Professor Van't Hoff, and a motion was made and unanimously carried authorizing the chairman and executive committee to take such measures and make such preparations as might be required for entertaining Professor Van't Hoff.

The meeting then adjourned until October.

DURAND WOODMAN,
Secretary.

DISCUSSION AND CORRESPONDENCE.

EBBINGHAUS'S THEORY OF COLOR-VISION.

IN proposing his specialization of Hering's theory of color-vision, Ebbinghaus had for an object to give it a basis in fact by showing a connection between the kind of light which must be absorbed by the colored substances in the retina and the subjection distribution of color throughout the spectrum as revealed by color-equations—especially those of the partially color-blind. The connection was a very forced one from the beginning: the visual yellow could stand very well as the absorbent of the light necessary to the production of the sensation blue, but the visual purple ought to have been *blue* in color to fulfill its function of absorbent of the light that causes the sensation *yellow*, while in reality it is not even purple (in the English sense of the word), but magenta; and the existence of a visual green and a visual red was purely hypothetical. Thus of the four colored absorbent substances to which so

fundamental a rôle was assigned, two (the red and the green) did not exist at all so far as known, and one existed only in a wholly erroneous color, and, moreover, in the fovea where vision is most acute, not even the visual yellow, the sole carrier of so large an assumption, has ever, by the most careful methods, been detected; it would seem to be far simpler to suppose that the objective color of the absorbent medium has nothing to do with the case.

The proof which was furnished by König immediately after the Ebbinghaus theory was proposed that the absorption of the visual purple is exactly what is needed to account for that increment in vision which is gradually acquired upon the oncoming of darkness, rendered the theory, of course, far more untenable still, and in fact Professor Ebbinghaus himself seems to be no longer inclined to insist upon it. He says (*Grundzüge der Psychologie*, I., 261, 262): "Ich habe vor einiger Zeit einen Versuch in dieser Hinsicht gemacht und darauf hingewiesen, dass zwischen der Art, wie die Farbenblinden Gelb und Blau im Spectrum verteilt sehen, und der Lichtabsorption des Sehpurpurs und des Sehgelb (nach der Untersuchungen Kühnes) eine auffellende Aehnlichkeit bestehe. Da nun offenbar die Bewusstseinswirkung der optischen Reize durch eine den Eindrücken entsprechende Absorption der verschiedenen Lichtstrahlenvermittelt werden muss, so nahm ich an, dass eben in dem Sehpurpur die Heringsche Blaugelbsubstanz zu erblicken sei.

"Ich sehe jedoch davon ab, die an diesen Ausgangspunkt angeschlossenen und zum Teil davon gang unabhängigen Gedanken hier zu wiederholen, weil sich bei genauerer Untersuchung des Sehpurpurs durch A. König meine ihn betreffende Annahme nur teilweise bestätigt fand. Das Sehgelb allerdings zeigte in dem einzigen Falle, in dem es erhalten werden konnte, eine mit der Blauempfindung der Farbenblinden annähernd übereinstimnende Lichtabsorption. Die Lichtabsorption des Sehpurpurs selbst dagegen entsprach vielmehr der Verteilung der Helligkeiten in dem Dunkelspectrum des normalen Anges, d. h. also auch in dem Spectrum der total Farbenblinden."

To refrain from reproducing the theory in its author's own text-book of psychology is probably to be regarded as tantamount to withdrawing it.

C. L. FRANKLIN.

SHORTER ARTICLES.

NOVA PERSEI, No. 2.

AN examination of the Draper photographs of the spectra of Nova Persei, No. 2, by Mrs. Fleming, shows that, like other novæ, it has been gradually changing into a gaseous nebula. The resemblance to the nebula N. G. C. 3918 is now so close that in a photograph taken on June 19, 1901, no marked difference was noted, except that the nebular line, 5007, is about eight times as bright as Hα in the nebula, and only equal to it in the nova. The lines 3869, 3970 (Hη), 4102 (Hδ), 4341 (Hγ), 4688, 4862 (Hβ), 4950, and 5007, are common to both and, except the last, have nearly the same relative intensities. Four bright lines between Hγ and Hβ appear faintly in the nova and are not present in the nebula, while one, 4364, is seen in the nebula but not in the nova, perhaps owing to the proximity of Hγ.

EDWARD C. PICKERING.

HARVARD COLLEGE OBSERVATORY,
CAMBRIDGE, MASS., June 25, 1901.

LIME AND MAGNESIA IN PLANT PRODUCTION.

SINCE 1899, the writer, with Dr. O. Loew, of the Division of Vegetable Physiology and Pathology of the U. S. Department of Agriculture, has been carrying on a series of experiments on the relation of lime to magnesia in the growth of plants. Some very interesting results have been attained which are to be published in a Bulletin of the Division to be issued at an early date. It may be of interest to here set forth a few of those results.

It is well known that magnesium salts form some of the more noxious alkali soils of the arid regions. In other sections it has been noticed that the soils well fertilized, especially with certain crude potash salts, after a time fail to respond to the fertilizers applied and either become sterile or their productive capacity is greatly reduced. This is apparently due to the accumulation of magnesia in the soil, it being present in some potash salts to a great

extent. One examined by the writer contained 9.37 per cent. of this element.

In our experiments in water, sand and soil cultures covering a great many trials we found that magnesia in a soluble form in very small amounts was, in the absence of lime, very toxic to plants. By the addition of lime in a soluble form in amount equal to or in slight excess of the magnesia the poisonous effect of the latter was eliminated. The plant was even enabled to withstand any bad effects from a greatly increased amount of magnesia, provided the lime was also increased in an equal or greater degree.

While the addition of other salts to the magnesia in the culture solutions variously affected the toxicity, in no case as far as tested did it have any degree of action comparable with that of the lime salts.

In sand cultures in which the lime and magnesium salts were applied as nitrates the plants made the better growth where the lime was slightly in excess of the magnesia. With lime greatly in excess there was apparently a starvation of the plant, it making a straggling growth for awhile. With the magnesium in excess the plant soon succumbed, and in moderate excess made a slow and unwholesome growth.

In soil cultures in which magnesium carbonate had been added, calcium carbonate was not efficient in overcoming the noxious influence of the magnesia. This was undoubtedly due to the less solubility of the calcium carbonate in comparison with the magnesium carbonate. By the addition of calcium sulphate the toxicity of the magnesia was readily overcome.

In the practical application of lime for its physiological effect the process could be more intelligently carried on where the amount of soluble magnesia and lime already present, *i. e.*, the portion immediately available to plants, is known. However, the danger lies in under- rather than in over-liming. In a soil containing CaO 0.144 per cent. and MgO 0.144 per cent. soluble in 1.115 per cent. of hydrochloric acid the addition of 0.8 per cent. of gypsum, and again of 0.8 per cent. of gypsum and 0.2 per cent. of magnesium carbonate, produced plants similar to those in the natural soil. With the addition of gypsum 0.2 per cent. and magnesium carbonate 0.68 per cent. the plants made a very slow and spindling growth, too much so for profitable crops.

In liming soils for the physiological effect the sulphate is, among the more soluble forms, the best available. Where a large amount is to be added on account of a high magnesia content of the soil it is better to add small portions yearly, as has been pointed out. However, the process should be repeated until the lime content of the soil is made to equal the total magnesia content.

As stated by Wheeler in his work on Rhode Island soils, calcium carbonate is the best form of lime for overcoming an acid reaction. In applying it for this or any other purpose care should be taken that magnesian limestone be not used unless the soil is also deficient in magnesia. Again in the continued application of crude potash salts the lime and magnesian content of the soil should be known. In case there is a deficiency of lime in the soil the addition of gypsum should also be made in some excess of the magnesia contained in the fertilizers.

D. W. May.

U. S. Department of Agriculture.

QUOTATIONS.

INDUSTRY AND RESEARCH.

Lord George Hamilton has written to Sir Alfred Hickman, M.P., ex-president of the British Iron Trade Association, explaining why certain contracts were placed by Indian railway companies with American firms. In the course of his remarks he says: "You seem to think that orders have only gone abroad because those who gave them did not understand their business. I wish that it were so. The competition we have to face is founded on something much more formidable and substantial. Chemical research, concentration of capital, thorough technical education, improved industrial organization have made in recent years greater advance in America than here; it is with the product of these combinations and not with the assumed stupidity of Indian officials that the British engineer has to contend." Sir Alfred

Hickman replies in a long letter, which appeared in the *Times*, but his remarks refer more to alleged imperfections in American work and the value of protection than to the cause of competition. He asks what evidence exists of 'superior chemical research, technical education, etc.,' and says, "I deny the 'chemical research'" mentioned by Lord George Hamilton. Apparently Sir Alfred Hickman attaches no importance to such reports as those prepared for the University of Birmingham and the Manchester Technical Education Committee as to the position of technical education in the United States; and he can scarcely be familiar with American scientific and technical publications or he would not 'deny the chemical research' with so free a mind. It seems pretty clear, however, that the India Office official who wrote Lord George Hamilton's letter to Sir Alfred Hickman was not the one who expressed views about the chemistry at Coopers Hill and aided the efforts which have strangled the technical education of the officers of the Indian Public Works Department.—*Nature.*

CURRENT NOTES ON METEOROLOGY.

CANNON-FIRING TO PREVENT DAMAGE BY HAIL.

A MOST unfortunate report was recently made to the State Department by the United States Consul at Lyons, France. According to the summary of the report published in the daily papers, the consul states that great success has attended the experiment of firing cannon as a means of protecting orchards and vineyards from damage by hail, and goes on to say that cannon firing is now to be used in order to prevent or to lessen injury by frost. As Consular Reports are official documents, and are looked upon by most persons as authoritative, many inquiries naturally came in to the Department of Agriculture, in Washington, as to when the United States Government intended to adopt, or to experiment with, some such method of protection. It therefore became necessary that some official answer should be made to these inquiries, and by direction of the Secretary of Agriculture, the Chief of the Weather Bureau recently issued a statement to the press in which he says that it is his conviction that "we have here to do with a popular delusion as remarkable as the belief in the effect of the moon on the weather. * * * The great processes going on in the atmosphere are conducted on too large a scale to warrant any man or nation in attempting to control them. * * * After the experience that this country has had during the past ten years with rain-makers, I am loth to believe that the bombardment of hailstorms will ever be practiced, or even attempted, in the United States, much less encouraged by the intelligent portion of the community. Every effort should be made to counteract the spread of the Italian delusion which has been imported into this country."

On this recently much-debated question as to the possibility of preventing hail by means of cannonading, Van der Linden, in *Ciel et Terre* for May 16 sums up the discussion about as follows: "We see, on the one side, many who believe in the new method; on the other side, sceptics and those who are opposed to the method are calling for clearly established facts before they commit themselves, one way or the other. Under the circumstances, it seems wisest to await further developments before forming an opinion."

CLIMATE OF MANILA.

METEOROLOGICAL data lately published by the Jesuit Observatory, at Manila, are based on pressure, temperature and humidity observations made during the years 1883–1898, and rainfall observations during 1865–1898. The normal temperatures, relative humidities and rainfall for each month follow:

	Temperature. Fahr.	Relative Humidity. Per cent.	Rainfall. Inches.
Jan.	77.0	77.7	1.193
Feb.	77.7	74.1	0.413
March	80.4	71.7	0.736
April	82.9	70.9	1.142
May	83.3	76.9	4.197
June	82.0	81.5	9.622
July	80.8	84.9	14.567
Aug.	80.8	84.4	13.866
Sept.	80.4	85.6	14.925
Oct.	80.4	82.6	7.536
Nov.	79.0	81.6	5.126
Dec.	77.4	80.7	2.134

EQUINOXES AND STORM WINDS.

At the May meeting of the Royal Meteorological Society (London) Mr. Rupert T. Smith read a paper on 'The Periodicity of Cyclonic Winds,' which was a discussion of his own observations made in the neighborhood of Birmingham during the years 1874–1890. The equinoxes do not appear to be very stormy periods, but the greatest frequency and force of cyclonic winds occurs some two weeks before the spring equinox and some three weeks after the autumn equinox.

R. DeC. Ward.

BIOLOGICAL SURVEY OF THE GREAT LAKES BY THE UNITED STATES FISH COMMISSION.

The United States Fish Commission will continue during the present summer the Biological Survey of the Great Lakes, inaugurated in 1898. The writer withdraws temporarily from the active management of the enterprise, and the Survey has been placed for the summer under the direction of Professor H. S. Jennings of the University of Michigan, and Professor Henry B. Ward of the University of Nebraska. Active work begins June 15.

Professor Ward, with an assistant, will continue the investigations on the plankton and plankton methods carried on during previous summers.

The remainder of the work, under the immediate charge of Dr. Jennings, will have headquarters at Put-in-Bay, Ohio, on Lake Erie, although the different investigations will be carried on at such points on the lakes as are most favorable. The following is a list of the investigators who will be at work, together with the lines of research which will be carried on:

Professor H. S. Jennings, of the University of Michigan: the movements and reactions of the plankton organisms.

Professor F. C. Newcombe, of the University of Michigan, in general charge of the botanical work: physiological investigations into the relations of the lake plants to the water and substratum.

Professor R. H. Pond, of the Maryland Agricultural College: the distribution of plants and soils at the west end of Lake Erie.

Professor Julia Snow, of Rockford College: the lake Algæ.

Professor S. O. Mast, of Hope College: the breeding habits of the sturgeon.

Mr. Raymond Pearl, of the University of Michigan: a statistical study of the races of whitefish and wall-eyed pike.

Mr. Leon J. Cole, of the University of Michigan: a study of the biology and feeding habits of the introduced carp, with especial reference to their supposed destruction of the eggs of other fish.

Professor Chas. Fordyce: systematic work on the Cladocera.

Mr. H. W. Graybill, of the University of Nebraska: the Echinorhynchi of the lake fish.

The University of Michigan cooperates with the Survey by allowing the use of its extensive library of the fresh-water fauna and of certain apparatus. The U. S. Fish Hatchery at Put-in-Bay will be fitted up as a working laboratory, and the steamer *Shearwater*, belonging to the Put-in-Bay station, will be employed in some of the investigations undertaken.

Jacob Reighard.

Ann Arbor, Mich., June, 1901.

THE JUBILEE OF THE UNIVERSITY OF GLASGOW.

One of the most interesting events in connection with the recent celebration at Glasgow was Lord Kelvin's oration on James Watt and Sir Joseph Hooker's address in connection with the opening of the new botanical department.

As reported in the London *Times*, Lord Kelvin said:

"The name of James Watt was famous throughout the whole world, in every part of which his great work had conferred benefits on mankind in continually increasing volume up to the present day. It was fitting that the University of Glasgow, in this celebration of its ninth jubilee, should recollect with pride the privilege it happily exercised 145 years ago of lending a helping hand and giving a workshop within its walls to a young man of no University education, struggling to begin earning a livelihood as a mathematical instrument-maker, in whom was then discovered something of the genius destined for such great things in the future. In a note by Watt appended to Professor Robison's dissertation on steam engines, he said that his attention was first directed in

the year 1759 to the subject of steam engines by the late Dr. Robison, then a student in the University of Glasgow and nearly of his own age. He at that time threw out an idea of applying the power of the steam engine to the moving of wheel carriages and to other purposes, but the scheme was not matured, and was soon abandoned. On his going abroad about the year 1761 or 1762, Watt tried some experiments on the force of steam in a Papin's digester, and formed a species of steam engine by fixing upon it a syringe one-third of an inch in diameter with a solid piston, and furnished also with a cock to admit the steam from the digester or shut if off at pleasure, as well as to open a communication from the inside of the syringe to the open air, by which the steam contained in the syringe might escape. That single-acting, high-pressure syringe engine, made and experimented on by James Watt 140 years ago in his Glasgow College workshop, now in 1901, with the addition of a surface condenser cooled by air to receive the waste steam and a pump to return the water thence to the boiler, constituted the common road motor, which, in the opinion of many good judges, was the most successful of all the different forms tried within the last few years. Watt left Glasgow in 1774 to live in the neighborhood of Dr. Erasmus Darwin, grandfather of Charles Darwin. But Greenock and the University and city of Glasgow never lost James Watt. The University conferred the honorary degree of LL.D. upon him in 1806. In 1808 he founded the Watt Prize in Glasgow College. He became Fellow of the Royal Society of Edinburgh in 1784, Fellow of the Royal Society of London in 1785, correspondent of the French Academy of Sciences in 1808, one of the eight 'Associés Etrangers' of the French Academy of Sciences in 1814. He did not know if any university in the world ever had a tradesman's workshop and saleshop within its walls, even for the making and selling of mathematical instruments prior to 1757. But whether the University of Glasgow was or was not unique in its beneficent infraction of usage in this respect, it was certainly unique in being the first British University—perhaps the first university in the world—to have an engineering school and professorship of engineering. This began under Professor Lewis Gordon about 1843. Glasgow was certainly the first university to have a chemical teaching laboratory for students started by its first professor of chemistry, Thomas Thomson, some time between 1818 and 1830. Glasgow was also certainly the first university to have a physical laboratory for the exercise and instruction of students' experimental work, which grew up with very imperfect appliances between 1846 and 1856. Pioneer though it was in those three departments, it had been outstripped within the last ten or fifteen years by other universities and colleges in the elaborate buildings and instruments now needed to work effectively for the increase of knowledge by experimental research and the practical instruction of students. But there was no lagging to-day in the resolution to improve to the utmost in all affairs of practical importance, and they almost saw attainment of the further aspirations to excel over all others in the magnificent James Watt Engineering Laboratory of the University of Glasgow to be ready for work before the expected meeting of the Engineering Congress next September. Now, through the magnificently generous kindness of Mr. Andrew Carnegie to the people among whom he has made for himself a summer home in the land of his birth, all the four Scottish universities could look forward to a largely increased power of benefiting the world by scientific research and by extending their teaching to young people chosen from every class of society as likely to be made better and happier and more useful to our country by university education."

In the course of his address Sir Joseph Hooker said:

"The audience might imagine themselves carried back to the first quarter of the last century when his father was professor of botany. He had not been educated for the medical, or, indeed, any other learned profession. Having inherited ample means and having been from childhood devoted to the study and collection of objects of natural history, he determined to devote his life and his fortune to travel and scientific pursuits. Early in 1820, reduced circumstances requiring him to turn

his botanical attainments to material account, he obtained, through the influence of his friend, Sir Joseph Banks, with George III., the chair of regius professor of botany in this university. It was a bold venture for him to undertake so responsible an office, for he had never lectured, or even attended a course of lectures, and in Glasgow, as in all other universities in the kingdom, the botanical chair was, and had always been, held by a graduate in medicine. Owing to these disqualifications his appointment was naturally unfavorably viewed by the medical faculty of the University. But he had resources that enabled him to overcome all obstacles—familiarity with his subject, devotion to its study, energy, eloquence, a commanding presence, with urbanity of manners, and, above all, the art of making the student love the science he taught. After 20 years of the professorship his father retired and undertook the directorship of the Royal Gardens, Kew. Since that period great changes had been introduced in the method of botanical teaching in all our universities, due, on the one hand, to a vastly advanced comprehension of the structure of plants and of the functions of their organs, and, on the other, to a recognition of the fact that the study of the animal and vegetable kingdoms could not be considered apart. Furthermore, chemistry, physics, and greatly improved microscopes were now necessary for the elucidation of the elementary problems of plant life. The addition of the building in which they were assembled was evidence of the resolve that botany should not fall from its well-earned position. The botanical laboratory would prove an invaluable aid to research under the ægis of its distinguished director, and in that belief he now declared it open.

THE HARVARD CHEMICAL LABORATORY.

Professor T. W. Richards writes in the last number of the *Harvard Graduate Magazine* in regard to research work, as follows:

"Original investigation, which has added so much to the intellectual life of the Laboratory, continues with unabated vigor. In the last five years about seventy papers have been published by the officers and students in Boylston Hall. These covered a wide range of subjects, about half of them concerning organic chemistry, and the other half physical and inorganic chemistry. Professor Jackson's extended researches upon the structure of aromatic substances have yielded in the hands of many students a large number of interesting new compounds and the basis for further generalization upon the mechanism of chemical action. Professor Hill's precise and detailed study of pyromucic acid has now in part given place to an extremely interesting series of syntheses of the benzol ring. In physical chemistry several comprehensive papers on chemical thermodynamics have appeared, and various phenomena were studied in the Laboratory by both instructors and students. For example, the passage of electricity through gases received attention; modern theory of dissociation was studied in its relation to the sense of taste; a new basis for thermometric standardization has been found; and the fundamental Law of Faraday has been subjected to a verification more rigorous than ever before. The study of the law of definite proportions, the one other chemical law which seems to rank with Faraday's in unfailing precision, has been steadily continued. In the last ten years the atomic weights of copper, barium, strontium, calcium, zinc, magnesium, cobalt, nickel, uranium and cæsium have all been studied with a care which seems to carry conviction with it. This work has all been handicapped by the inadequate quarters in which it had to be performed, and we now have to face the bitter alternative of being obliged either to turn away graduate students, or else so to crowd them together as to make accurate investigation almost impossible."

SCIENTIFIC NOTES AND NEWS.

Harvard University has conferred the LL D. degree on Dr. H. S. Pritchett, president of the Massachusetts Institute of Technology; on Professor J. H. Van't Hoff, professor of physical chemistry at the University of Berlin, and on Professor C. S. Sargent, director of the Arnold Arboretum. The honorary M.A. was conferred on Dr. Hugo Münsterberg, professor of psychology, and on Dr. Theobald Smith, professor of comparative pathology. In conferring these last degrees, President Eliot referred to the

recipients as follows: "Professors in this University, not of Harvard nurture, distinguished for productive research—to make them children of the house."

YALE UNIVERSITY, at its commencement exercises last week, conferred the LL.D. on Dr. E. B. Wilson, professor of zoology in Columbia University. It will be remembered that the same degree was given to Professor Wilson by the University of Chicago the previous week. The honorary M.A. was conferred on Mr. Gifford Pinchot, B.A. (Yale, '89), chief of the Bureau of Forestry, U. S. Department of Agriculture.

TRINITY COLLEGE has conferred the LL.D. degree on Professor W. H. Howell, professor of physiology in the Johns Hopkins University, and on Professor F. H. Osborn, professor of zoology in Columbia University and curator in the American Museum of Natural History. It will be remembered that Professors Howell and Osborn gave the chief addresses on the occasion of the dedication of the Hall of Natural History last winter.

BOWDOIN COLLEGE has conferred its LL.D. on Dr. Alpheus S. Packard, professor of zoology and geology at Brown University.

AT the recent celebration of its ninth jubilee, the University of Glasgow conferred its LL.D. degree on 120 delegates from various universities and institutions. These included J. Mark Baldwin, professor of psychology at Princeton University; William G. Farlow, professor of cryptogamic botany at Harvard University; Dr. Adolph Meyer, lecturer on psychiatry at Clark University, and R. Mark Wenley, professor of philosophy at the University of Michigan. The foreign men of science on whom the degree was conferred are: Professor Karl Bezold, University of Heidelberg; Professor W. C. Brogger, University of Christiania; Professor Daniel J. Cunningham, Trinity College, Dublin; Professor Nicolas Egoroff, Imperial Military Academy of Medicine, St. Petersburg; Principal George Carey Foster, University College, London; Sir Archibald Geikie; Professor A. A. W. Hubrecht; Professor Kronecker, University of Berne; Dr. Joseph Larmor, St. John's College, Cambridge; Principal Oliver J. Lodge, University of Birmingham; President James Loudon, University of Toronto; Sir William MacCormac; Professor James G. MacGregor, Nova Scotia; Professor John Perry, Royal College of Science, London; Professor F. York Powell, Oriel College, Oxford; Sir Henry S. Roscoe, Vice-Chancellor of the University of London; Professor Joseph J. Thomson, University of Cambridge; John Isaac Thornycroft, naval architect and engineer.

SURGEON-GENERAL GEORGE M. STERNBERG has left for the Philippine Islands on a tour of inspection.

DR. CH. WARDELL STILES, of the Bureau of Animal Industry, has been appointed a delegate from the U. S. Government to the International Congress of Zoologists which meets at Berlin next August.

PROFESSORS NICHOLAS SENN and D. R. Brower, of the Rush Medical College, University of Chicago, and Professor Jacob Frank, of the Chicago Polyclinic, have left New York for a trip round the world by the way of Siberia. It is expected that only fifty-nine days will be required for the traveling and one month will be spent at stopping places. They have been invited to hold surgical clinics in Moscow and Japan.

MAJOR ROSS, of the Liverpool School of Tropical Medicine, and Dr. Logan Taylor, assistant professor of pathology at Glasgow University, sailed on June 15 from Liverpool for West Africa to continue researches on malaria.

A PARTY from the U. S. Geological Survey, including Mr. W. C. Mendenhall, geologist, and L. E. Reaburn, topographer, have left Dawson for explorations in northern Alaska.

DR. S. WEIR MITCHELL, of Philadelphia, has arrived in California after a trip round the world.

DR. OSCAR UHLWORM, of Cassel, editor of the *Botanisches Centralblatt*, has been appointed director of the German Bureau for the International Catalogue of Scientific Literature.

A WALKER PRIZE, of the Boston Society of Natural History, has been awarded to Mr.

Edward W. Berry, of New York, for a memoir on *Liriodendron*.

Professor St. John, of Oberlin College, and Professor Nichols, of Dartmouth College, will spend the summer working at Yerkes Observatory.

The professors of chemistry at Harvard University, Professors Jackson, Hill, Sanger and Richards, gave a dinner to Professor van't Hoff, of the University of Berlin, on June 27. Among the invited guests from a distance were Professor Michael, of Tufts College, and Professor Bancroft, of Cornell University.

The Rev. Joseph Cook died at Ticonderoga, New York, on June 24. Some years ago Mr. Cook attracted much attention by his Boston Monday lectures on religion and science, published under titles such as 'heredity,' 'biology,' etc.

Dr. Sven Hedin has returned to Chaklik after a successful journey through the Desert of Gobi. He is now continuing his travels across Thibet.

The members of the French commission entrusted with the measurement of an arc in Peru arrived at Guayaquil on June 1, and will commence their observations during the present month. They will be joined by M. Gonnessiat, who, a year ago, took charge of the observatory at Quito.

According to the N. Y. *Evening Post* E. A. Martel, the French explorer of caverns, whose discoveries underground have attracted much attention, reports that he has found in the department of Hautes Alps a cavity in the form of a 'natural well,' whose depth exceeds that of any other known. He has sounded it to the depth of about 1,027 feet, but the actual bottom has not been reached.

King Charles of Portugal and the members of the Portuguese Cabinet opened at Horta, in the Azore Islands, on June 29, the meteorological observatory in connection with the Weather Bureau at Washington.

In the fire at Jacksonville, Fla., the herbarium of Mr. A. H. Curtis, containing about 16,000 sheets, was destroyed.

A second Livingstone Exhibition was opened in London, on June 18. In addition to a collection of relics of the great explorer, lent by the Geographical Society and other contributors, there is a display of all kinds of articles conducing to the health or comfort of travelers in uncivilized regions. Not only are the special requirements of travelers represented, such as camp equipment, portable foods, clothing and general outfit, but other sections are devoted to sanitation, house-building and various matters of interest to residents in such regions. In connection with the Exposition a reception was given by the Royal Geographical Society on the eighteenth. During the evening, lectures were given in the council chamber, with the aid of the lantern, by Dr. Cornish, on 'Waves in Air, Water, Sand and Snow'; Dr. Francisco Moreno, on the 'Scenery of Argentina'; Dr. Manson, on 'Recent Malarial Discoveries'; Captain Barker, on the 'Antarctic Ship Discovery'; and Dr. Mills, who exhibited some recent geographical photographs.

At the last International Congress of Librarians, held at Paris, Mlle. Marie Pellechet established two prizes, of the value respectively of 1,000 and 500 fr. for the best memoirs relating to the insects that destroy books. The memoirs should be sent to M. Henry Martin, secretary of the congress, rue de Sully I, Paris.

The Rev. W. M. Beauchamp, Syracuse, N. Y., is continuing his series of archeological bulletins, published by the New York State Museum. The sixth bulletin on 'Bone and Horn Articles' is in press, and Mr. Beauchamp is now at work on metallic implements and ornaments. There will be figures of most of the native copper articles known in New York, and Mr. Beauchamp requests collectors of such articles to communicate with him.

The census returns show the population of France to be about 38,600,000, an increase of 330,000 since 1896. To this small increase Paris and its suburbs contribute 292,000, the greater part of which number is due to foreign immigrants, so that the rest of France gives an increase of only 38,000. Since 1850 the population of France, allowing for alterations of boundaries, has only increased from 35,000,000

to 38,000,000, whereas that of the United Kingdom has risen from 27,000,000 to 41,000,000, Germany from 35,000,000 to 56,000,000, Austria from 30,000,000 to 45,000,000, and Italy from 23,000,000 to 32,000,000.

MAJOR-SURGEON W. C. GORGAS, chief sanitary officer of Havana, has presented his report for May, showing a very gratifying state of affairs in Havana. For the first time in its history, there was not a single case of yellow fever in Havana on June 1, and there was only one death from this disease in the preceding three months. Regarding the two slight outbreaks of yellow fever, Major Gorgas writes: Since March 8 outbreaks of yellow fever have occurred twice. The first time, April 21 and 22, we had two cases; and again, on May 6 and 7, four cases. Each time the infected houses and three or four contiguous houses on every side of the infected house were carefully gone over. Every room in each of these houses was closed and sealed, and insect powder burned in them at the rate of one pound to 1,000 cubic feet. All standing water was drained away where possible, and oiled where it could not be drained. The results look as if the focus of infection at that particular point had been eradicated. In the case of the patient taken sick March 8, after our disinfection, we went forty-two days till the next case. Then an outbreak occurred, April 21 and 22. We again disinfected, and went fifteen days till the next cases, May 6 and 7. We again disinfected, and have gone twenty-four days without a case. I am more particularly impressed by these figures, as we commenced our systematic destruction about the middle of February. Formerly we paid no particular attention to the mosquito, merely disinfecting for yellow fever, as we do for other infectious diseases. The only part of the process that killed the mosquito was the formaline used in one or two infected rooms.

THE Medical Society of the State of California has passed the following resolution:

Whereas, It has been shown by our local bacteriologists, and by the Commission sent by the United States Government, that the bubonic plague has existed in San Francisco, and probably does at the present time; therefore, be it

Resolved, That the Medical Society of the State of California express its confidence that the San Francisco Board of Health, the State Board of Health, and the United States Commission will be able to watch the disease, and to take proper measures for its suppression.

THE Imperial Russian Association of Fisheries will hold an international exposition in February and March, 1902, at St. Petersburg, for the purpose of showing the condition of the fresh and salt water fisheries of the world. The expense of the exposition will be defrayed by the association, the crown, the municipal government, private contributions, and by charges for exhibition space and for the admission of visitors. Premiums will be awarded in the form of gold, silver and bronze medals, diplomas of honor and money prizes. The exposition will have nine departments, as follows: (1) Fisheries in general; (2) salt and fresh water fisheries; (3) implements used in the fisheries industry; (4) products of the fisheries; (5) manner and means for preserving fish; (6) arrangement of fish hatcheries; (7) fishing sport; (8) aquariums and their inmates; (9) scientific researches concerning the lives of fishes, etc.

UNIVERSITY AND EDUCATIONAL NEWS.

MR. J. PIERPONT MORGAN has undertaken to erect for the Harvard Medical School three new buildings at an estimated cost of $1,000,000. They are to be a memorial to his father, Mr. J. S. Morgan, who began his business career in Boston. This great gift was the result of an interview with Professors H. P. Bowditch and J. C. Warren last winter, but was communicated by cable from England at the recent commencement exercises. The Harvard Medical School can now be removed from its present site in Boston to Brookline, where twenty-one acres of land have been secured. The three buildings to be erected are for administration, for anatomy, histology and embryology, and for physiology and physiological chemistry.

IT was announced at the recent commencement exercises of Yale University that it was expected that the bicentennial fund of $21,000,000 would be collected by next autumn. The

sum required for land and buildings, $1,185,000, has been entirely secured. The subscribers to the fund number about seventeen hundred.

Mr. Ezra J. Warner, of Chicago, has given $12,500 to Middlebury College for the furnishing of Warner Science Hall, erected last year at a cost of over $80,000. The college has also received a gift of $5,000 from Dr. M. Allen Starr, of New York, to purchase books for the library.

The cornerstone of the new science building at Drury College, Missouri, was laid on June 13. Toward the cost of this building Dr. D. K. Pearsons contributed $25,000, and an equal amount has been made up by general subscription.

Senator Hanna has given $50,000 to Kenyon College, Gambier, O., for a dormitory.

John D. Rockefeller has promised $25,000 to Des Moines College, Iowa, on condition that friends of the institution raise $55,000 more.

The College of Physicians and Surgeons, Chicago, which is the medical department of the University of Illinois, has been destroyed by fire resulting from lightning. The loss on the building and equipment is said to be $200,000.

The commission selected to draft plans for the Carnegie Technical School at Pittsburg has presented its report. The commission proposes three divisions, the Carnegie Technical College, the Carnegie Technical High School and the Carnegie Artisan Day and Evening Classes. It advises that experimental shops and laboratories be built, that the college support one or more publications, to give the fruits of its research to the world; that English, French, and German and Spanish be studied. It also recommends courses of study for the three divisions. The commission consists of Professors V. C. Alderson, of Chicago, Robert H. Thurston, of Cornell University, Thomas Gray, of Terre Haute, Ind., and J. B. Johnson, of Wisconsin University.

Dr. F. W. Sanders has been compelled to resign the presidency of the New Mexico College of Agriculture and Mechanic Arts (Mesilla Park, N. M.). The following abstract from his letter of resignation has been published in the *New Mexico Collegian* for June, 1901: "I have declined to serve you longer unless you would abandon the pernicious system of annual elections, and have urged you to give this system up for a number of reasons; because, among other things, it almost inevitably lends itself to the control of the pettiest of personal and local and political influence, and makes a permanent policy impossible; but especially because it makes possible the removal of able and faithful employés for reasons that will not bear the light." The new president has not yet been elected.

The University of New Mexico, at Albuquerque, N. M., has been unfortunate in losing its president, Dr. C. L. Herrick, who resigned on account of ill health. Dr. Herrick is at present in the field, continuing his important researches in the geology of New Mexico.

Dr. Taliaferro, an instructor in the Pennsylvania State College, has been made President of the Florida Agricultural College.

At the University of Colorado, a chair of geology has been established which will be filled by Dr. N. M. Fenniman. The chair of philosophy, vacant by the death of Dr. Francis Kennedy has been filled by the appointment of Dr. M. F. Libby.

Karl E. Guthe has been promoted to an assistant professorship of physics at the University of Michigan.

Dr. A. P. Saunders has been promoted to a professorship of chemistry at Hamilton College.

S. P. Orth has been promoted to a professorship of natural science at Buchtel College.

Arthur L. Clark has been appointed professor of physics at Bates College.

Dr. Margaret K. Smith has been appointed professor of psychology at the State Normal School, New Platz, New York.

Dr. W. Palladin has been made professor of plant anatomy and physiology in the University of St. Petersburg.